AMERICAN SOCIETY
FOR
TESTING MATERIALS.

AFFILIATED WITH THE
INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

PROCEEDINGS

OF THE

ELEVENTH ANNUAL MEETING

Held at Atlantic City, New Jersey,
June 23-27, 1908.

VOLUME VIII.

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ATLANTIC CITY, N. J., JUNE 23–27, 1908.

THE ELEVENTH ANNUAL MEETING OF THE AMERICAN SOCIETY FOR TESTING MATERIALS was held at the Hotel Traymore, Atlantic City, N. J., on June 23–27, 1908. The total attendance at the meeting, including guests, was over 280.

The following members were present or represented at the meeting: H. Abraham; W. A. Aiken; Ajax Metal Company, represented by G. H. Clamer; American Bridge Company, represented by C. C. Schneider; American Foundrymen's Association, represented by Richard Moldenke; E. B. Ashby; Barrett Manufacturing Company, represented by William S. Babcock; William H. Bassett; T. J. Bateman; Florus R. Baxter; H. E. Beasley; H. B. Bent; H. C. Berry; Bethlehem Steel Company, represented by E. O'C. Acker and A. D. Mixsell; Robert M. Bird; John Birkinbine; A. Bonzano; Booth, Garrett and Blair, represented by Robert Job; C. W. Boynton; H. DeH. Bright; John G. Brown; Samuel A. Brown; William L. Brown; W. C. Bullitt; J. Cambier; Cambria Steel Company, represented by George E. Thackray; William Campbell; J. A. Capp; R. B. Carnahan, Jr.; Carnegie Steel Company, represented by J. O. Leech; F. D. Carney; G. H. Charls; Frank P. Cheesman; A. J. Christie; Sumner R. Church; F. O. Clements; McGarvey Cline; Colorado Fuel and Iron Company, represented by J. Cambier; E. A. Condit, Jr.; Frederick Conlin; R. D. Coombs; William A. Cooper; Frank H. Crockard; A. E. Crockett; O. C. Cromwell; Allerton S. Cushman; George C. Davies; Edward W. DeKnight; John Dewar; Cyril DeWyrall; H. E. Diller; D. E. Douty; A. W. Dow; Charles B. Dudley; W. O. Dunbar; Hubert Dunning; *Engineering Record*, represented by John M. Goodell; S. M. Evans; B. F. Fackenthal, Jr.; Henry Fay; A. I. Findley; Charles S. Foller; H. J. Force; C. N. Forrest; Wm. Forsyth; H. W. Foster; Henry A. Gardner; James H. Gibboney; Harry C. Gibson; G. M.

Goodspeed; H. S. Goodwin; R. S. Greenman; Norris B. Gregg; Ed. D. Gregory; J. E. Greiner; G. J. Griesenmauer; R. E. Griffith; H. Gulick; W. H. Harding; A. B. Harrison; H. J. Hartley; W. K. Hatt; G. B. Heckel; George B. Hemstreet; Milton L. Hersey; J. F. Hinckley; J. A. Holmes; James E. Howard; L. S. Hughes; Richard L. Humphrey; Joseph W. Hunter; Loren E. Hunt; Robert W. Hunt Company, represented by Robert W. Hunt; E. T. Ickes; Illinois Steel Company, represented by P. E. Carhart; F. P. Ingalls; *Insurance Engineering*, represented by A. Irving Brewster; *Iron Trade Review*, represented by A. O. Backert; H. L. James; J. Y. Jewett; Robert Job; E. F. Kenney; L. H. Kenney; J. A. Kinkead; H. G. Kittredge; Lackawanna Steel Company, represented by F. E. Abbott, and G. B. Waterhouse; Gaetano Lanza; T. R. Lawson; E. W. Lazell; R. W. Lesley; E. S. Lewis; John F. Lewis; Alfred Lovell; Lukens Iron and Steel Company, represented by H. Taggart; D. W. Lum; A. Lundteigen; T. D. Lynch; E. T. McCleary; A. S. McCreath and Son, represented by Andrew S. McCreath and Lesley McCreath; J. W. McGrady; Parker C. McIlhiney; Charles F. McKenna; D. W. McNaugher; R. W. Mahon; Edgar Marburg; Wm. Marshall; Charles A. Mead; Richard K. Meade; Rudolph P. Miller; Charles M. Mills; Richard Moldenke; A. W. Munsell; National Tube Company, represented by F. N. Speller; E. D. Nelson; Tinius Olsen, represented by Thorsten Y. Olsen; J. R. Onderdonk; L. W. Page; W. M. Parks; Pennsylvania Steel Company, represented by Wm. C. Cuntz and F. C. Kuntz; R. S. Perry; W. A. Polk; J. Madison Porter; Charles E. Price; Henry H. Quimby; *Railroad Gazette*, represented by W. H. Boardman; *Railway and Engineering Review*, represented by Willard A. Smith; J. C. Ramage; Reading Iron Company, represented by George Schuhmann; Clifford Richardson; George N. Riley; C. D. Rinald; S. M. Rodgers; Joseph Royal; C. C. Schneider; H. H. Scofield; W. F. Scott; Archie W. Schwartz; Charles Shannon; Samuel J. Shaw, Jr.; Jesse J. Shuman; C. E. Skinner; Hervey J. Skinner; Earl B. Smith; H. E. Smith; J. P. Snow; C. W. Sommerville; Henry S. Spackman Engineering Company, represented by Henry S. Spackman; C. R. Spare; C. E. Stafford; Standard Asphalt and Rubber Company, represented by James R. Valk; Standard Steel Works, represented by A. A. Stevenson;

W. F. Steffens; J. T. Stephenson; A. A. Stevenson; Bradley Stoughton; H. Taggart; H. P. Talbot; W. Purves Taylor; Rolf Thelen; A. R. Thomas; George Thomas, 3d; G. W. Thompson; Sanford E. Thompson; Harry D. Tiemann; Edwin B. Tilt; W. J. Tretch; J. M. Umstadter; United States Gutta Percha Paint Company, represented by Herbert W. Rice; C. P. Van Gundy; S. S. Voorhees; F. R. Wadleigh; Samuel Tobias Wagner; Percy H. Walker; L. W. Walter; H. E. Walters; George S. Webster; Wm. R. Webster; Thomas D. West; F. W. Weston; G. D. White; M. H. Wickhorst; Percy H. Wilson; Winchester Repeating Arms Company, represented by R. L. Penny; Alan Wood, 3d; R. D. Wood and Company, represented by Walter Wood; Walter Wood; J. E. Woodwell; Ira H. Woolson; J. Bertram Young.

Total number, 209 (including representations); total number in personal attendance, 202.

FIRST SESSION.—TUESDAY, JUNE 23, 3 P. M.

Business Meeting.

President Charles B. Dudley in the chair.

The minutes of the Tenth Annual Meeting were approved as printed.

The annual report of the Executive Committee was adopted as printed.

The Chair appointed Mr. M. H. Wickhorst and Mr. J. J. Shuman as tellers to canvass the ballot for officers.

A paper entitled "Testing is not Inspection," was read by Mr. W. A. Aiken.

The annual report of Committee B, on Standard Specifications for Cast Iron and Finished Castings, Walter Wood, Chairman, was read by the Secretary.

In pursuance of the recommendation of Committee B, the following resolutions were passed:

WHEREAS, The American Foundrymen's Association has asked for coöperation in bringing about specifications for pig iron on the basis of chemical analysis.

Resolved, That Committee B be authorized to join with

the American Foundrymen's Association, and similar associations in the preparation of specifications for foundry pig iron.

A paper entitled "Method of Obtaining a Truly Circular and Uniform Chill in Rolls," was presented by Mr. T. G. West.

The annual report of Committee H, on Standard Tests for Road Materials, was read by the Chairman, Mr. L. W. Page. The Committee recommended the adoption as standards of (1) the Abrasion Test for Road Materials, accompanying its report at the Seventh Annual Meeting (1904), and (2) of the Toughness Test for Macadam Rocks, accompanying its report at the Eighth Annual Meeting (1905).

On the motion of Mr. L. W. Page the adoption of these standards was referred to letter-ballot of the Society.

A paper on "The Acceptance of Stone for Use on Roads Based on Standard Tests," was read by Mr. R. S. Greenman.

A report on "Fuel Investigations, Geological Survey: Progress during the Year Ending June 30, 1908," by Mr. J. A. Holmes, was, in the absence of the author, presented by Mr. H. M. Wilson.

The following papers were then read:

"Commercial Results in the Purchase of Coal on Specifications." J. E. Woodwell.

"The Structural Timbers of the Pacific Coast." R. Thelen.

The tellers reported that 234 ballots had been cast, and in accordance with their report the Chair declared the election of Mr. C. B. Dudley, President; Mr. Robert W. Lesley, Vice-President; Mr. Edgar Marburg, Secretary-Treasurer, and Mr. James Christie, Member of the Executive Committee.

The following proposed amendments to the By-Laws, recommended by the Executive Committee, were referred to letter-ballot of the Society:

Insert the following new article:

PROCEDURE GOVERNING THE ADOPTION OF STANDARD SPECIFICATIONS.

ARTICLE IV.

SECTION 1. A proposed standard specification must be presented at the Annual Meeting, at which it may be amended by

majority vote of those voting. A two-thirds affirmative vote of those voting shall be required to refer the specification to letter ballot of the Society. A two-thirds affirmative vote of those voting on letter-ballot shall be required for the adoption of the specification.

Amend Article V (old Article IV), Section 1, by striking out "January" and substituting "July."

Mr. H. E. Beasley moved that the question of appointing a committee on standard tests of asphalt be referred to the Executive Committee.

The Secretary stated that the question was one within the province of Committee H, on Standard Tests for Road Materials, which Committee had made a partial report on the subject at the Eighth Annual Meeting. He accordingly suggested a substitute motion, to the effect that Committee H be instructed to present, if possible, a report on standard tests for asphalt at the next annual meeting.

This suggestion was accepted by the author of the original motion and the substitute motion was carried.

The meeting then adjourned till 8 P. M.

SECOND SESSION.—TUESDAY, JUNE 23, 8 P. M.

President Charles B. Dudley in the chair.

The Chairman invited the Vice-President, Mr. Robert W. Lesley, to the chair and read the Annual Presidential Address on "Some Features of the Present Steel Rail Question."

A paper on "The Relative Corrosion of Steel and Wrought Iron Tubing," by Mr. H. M. Howe and Mr. Bradley Stoughton, was read by Mr. Stoughton.

The annual report of Committee U, on the Corrosion of Iron and Steel, was presented by Mr. A. S. Cushman, Chairman.

A paper on "Electrolysis and Corrosion," was then read by Mr. A. S. Cushman.

This was followed by a general discussion on the subject of Corrosion of Steel and Wrought Iron.

A paper entitled "Characteristic Results of Endurance Tests

on Wrought Iron, Steel, and Alloys," by Mr. Henry Souther, was read by title.

The meeting then adjourned till the following morning.

THIRD SESSION.—WEDNESDAY, JUNE 24, 10 A. M.

President Charles B. Dudley in the chair.

A paper on "Some Practical Applications of Metallography" was presented by Mr. William Campbell and discussed.

The Preliminary Program of Tests of Steel Columns in Progress at Watertown Arsenal was presented by Mr. Edgar Marburg, followed by the presentation by Mr. J. E. Howard of the results of the tests thus far made.

The annual report of Committee T on the Tempering and Testing of Steel Springs and Standard Specifications for Spring Steel, was presented by Mr. J. A. Kinkead, Chairman.

A paper on "Tests of Staybolts," by Mr. E. L. Hancock, was read by title.

The annual report of Committee Q on Standard Specifications on the Grading of Structural Timber, Hermann von Schrenk, Chairman, was presented by Mr. W. K. Hatt, Secretary of the Committee.

A paper on "The Influence of the Absorptive Capacity of Brick upon the Adhesion of Mortar," was read by Mr. D. E. Douty and discussed.

The meeting then adjourned till 3 P. M.

FOURTH SESSION.—WEDNESDAY, JUNE 24, 3 P. M.

On Rails.

President Charles B. Dudley in the chair.

The President opened the session with the statement that since this meeting was to be devoted to a discussion of matters relating to rails, to which his annual address read on the evening before related, he desired to make it clear that the usual understanding that a presidential address was not open to discussion

would be cheerfully waived, and that any member should feel at liberty to make any comments on the same that he might desire.

The annual report of Committee A, on Standard Specifications for Iron and Steel, W. R. Webster, Chairman, was presented by Mr. Edgar Marburg, Secretary of the Committee.

This report embodied the recommendation that the Standard Specifications for Steel Rails be amended as follows:

Paragraph 3. Add the following sentence after the first sentence in the paragraph: For rails weighing 85 to, and including, 100 pounds per yard, one drop test shall be made from every blow of steel.

Paragraph 14, first line, change the sentence: "No. 2 rails will be accepted up to ten (10) per cent. of the whole order," to "No. 2 rails shall be accepted to at least five (5) per cent. of the whole order."

Add the following sentence at the close of the paragraph: Rails rejected under the drop test will not be accepted as No. 2 rails.

On motion of the Secretary these amendments were referred to letter-ballot of the Society.

The "Preliminary Program and Results of Work on the Metallurgy of Steel in Progress at Watertown Arsenal" was presented by Mr. J. E. Howard.

The following papers were then read:

"A Microscopic Investigation of Broken Steel Rails: Manganese Sulphide as a Source of Danger." Henry Fay.

"Rail Failures—Mashed and Split Heads." M. H. Wickhorst.

"Some Notes on the Rail Situation." E. F. Kenney.

A paper on "Some Results Showing the Behavior of Rails under the Drop Test and Proposed New Form of Standard Drop Testing Machine," by Mr. S. S. Martin, and one on "Types of Failures in the Base of Cold-Rolled Rails," by Mr. P. H. Dudley, were read by title.

The formal discussion on the general subject of Rails was then opened by Mr. W. R. Webster, Mr. J. P. Snow and Mr. R. W. Hunt.

The meeting then adjourned till the following morning.

FIFTH SESSION.—THURSDAY, JUNE 28, 10 A. M.

On Cement and Concrete.

Vice-President Robert W. Lesley in the chair.

The annual report of Committee C on Standard Specifications for Cement, G. F. Swain, Chairman, was presented by Mr. R. L. Humphrey, Secretary of the Committee. The report embodied certain proposed modifications of the Standard Specifications for Cement, which after discussion were passed to letter-ballot of the Society.

On motion of Mr. J. G. Brown a vote of thanks was passed in recognition of the labors of Committee C.

A paper entitled "Portland Cement Standards," by Mr. W. W. Maclay, was read by title.

The annual report of Committee I on Reinforced Concrete, F. E. Turneaure, Chairman, was read by Mr. R. L. Humphrey, Secretary of the Committee.

Then followed the reading and discussion of the following papers:

"Sands—Their Relation to Mortar and Concrete." H. S. Spackman and R. W. Lesley.

"Permeability Tests of Concrete with the Addition of Hydrated Lime." S. E. Thompson.

"Some Tests of Reinforced Concrete Beams under Oft-Repeated Loading." H. C. Berry.

The meeting then adjourned till 8 P. M.

SIXTH SESSION.—THURSDAY, JUNE 25, 8 P. M.

On Cement and Concrete.

Vice-President Robert W. Lesley in the chair.

The following papers were read and discussed:

"Formulas for Reinforced Concrete Beams in the Light of Experimental Data." W. F. Scott.

"Shearing Values of Stone and Concrete." H. H. Quimby.

"The Influence of Fine Grinding on the Physical Properties of Portland Cement." R. K. Meade.

"Cement and Concrete Works of United States Reclamation Service, with Notes on Disintegration of Concrete by Action of Alkali Water." J. Y. Jewett.

The following papers were read by title:

"Tests of Reinforced Concrete Block Sewer and Railway Culverts." Burton Lowther.

"Hydrated Lime and Cement Mortars." E. W. Lazell.

"Tests of Bond in Reinforced Concrete Beams." M. O. Withey.

The annual report of Committee D, on Standard Specifications for Paving and Building Brick, was read by the Chairman, Mr. L. W. Page.

The meeting then adjourned till the following morning.

SEVENTH SESSION.—FRIDAY, JUNE 26, 10 A. M.

On Preservative Coatings and Lubricants.

President Charles B. Dudley in the chair.

The annual report of Committee E on Preservative Coatings for Iron and Steel, was presented by Mr. S. S. Voorhees, Chairman.

Appendix I to this report, on Methods of Analysis, was read by Mr. P. H. Walker, and Appendix III, on Methods of Tests, by Mr. H. B. Potter, was read by title.

The following papers were then read:

"Certain Solubility Tests on Protective Coatings." G. W. Thompson.

"The Inhibitive Power of Certain Pigments on the Corrosion of Iron and Steel." A. S. Cushman.

"Analysis of Varnishes." P. C. McIlhiney.

The annual report of Committee N on Standard Tests for Lubricants, A. H. Gill, Chairman, was read by the Secretary.

Then followed a general discussion on the question "Will 'Pure-Paint' Legislation Give Us Better Paints?" opened by Messrs. C. D. Rinald, J. Dewar, J. Peters, and G. W. Thompson. A written discussion of the subject by Mr. E. F. Ladd was read by the Secretary.

The meeting then adjourned till 3 P. M.

EIGHTH SESSION.—FRIDAY, JUNE 26, 3 P. M.

On Testing Machines and Apparatus.

President Charles B. Dudley in the chair.

The following papers were read and discussed:

"The Use of an Extensometer in Commercial Work." T. D. Lynch.

"Special Features of a Recently Installed 600,000-pound Universal Testing Machine." T. Y. Olsen.

"New Forms of Pendulum Testing Machines." T. Y. Olsen.

The annual report of Committee R on Uniform Specifications for Boilers was read by the Chairman, Mr. E. D. Meier. On motion of the Secretary the recommendations embodied in this report were referred to Committee A, on Standard Specifications for Iron and Steel.

A paper entitled "An Autographic Recorder for Commercial Tension Tests," by H. F. Moore, was read by the Secretary.

A report on "The Structural Material Testing Laboratories, United States Geological Survey: Progress during the year ending June 30, 1908," was presented by Mr. R. L. Humphrey.

A paper on "Uniformity in Magnetic Testing and in the Specification of Magnetic Properties," was read by Mr. C. W. Burrows.

The meeting then adjourned till the following morning.

NINTH SESSION.—SATURDAY, JUNE 27, 10 A. M.

President Charles B. Dudley in the chair.

On motion of Mr. R. W. Hunt the Executive Committee was instructed to consider the desirability of increasing the testing facilities of the country by the construction of a testing machine of sufficient capacity for the testing of large full-sized members, and to recommend to the Society such action as in their judgment might seem best.

The following papers were then read and discussed:

"Forest Service Tests to Determine the Influence of Different Methods and Rates of Loading on the Strength and Stiffness of Timber.

(*a*) "The Purpose and Scope of the Investigations." McGarvey Cline.

(*b*) "Analytical Discussion of Speed-Strength Relation." H. D. Tiemann.

"Manganese Bronze." C. R. Spare.

"Notes on the Desirability of Standard Specifications for Hard-Drawn Copper Wire." J. A. Capp and W. H. Bassett.

On motion of Mr. J. A. Capp, the Executive Committee was instructed to consider the desirability of appointing a committee on standard specifications for hard-drawn copper wire.

A paper on the "Effect of Combined Stresses on the Elastic Properties of Steel," by Mr. E. L. Hancock, was read by title.

The annual report of Committee P on Fireproofing Materials, I. H. Woolson, Chairman, was read by Mr. R. P. Miller, Secretary of the Committee. On motion of Mr. Miller the modifications in the Standard Test for Fireproof Floor Construction proposed by the Committee were referred to letter-ballot of the Society.

The annual report of Committee S, on Waterproofing Materials, was presented by Mr. W. A. Aiken, Chairman.

The following papers were then read by title:

"Notes on the History of Testing Machines, with Special Reference to European Practice." J. H. Wicksteed.

"Testing Lubricating Oils," Henry Souther.

On motion of Mr. D. E. Douty a vote of thanks was expressed to the Executive Committee for the arrangement of the program at this meeting, by which the holding of parallel sessions was avoided.

The Chairman then declared the meeting adjourned *sine die*.

American Society for Testing Materials.

AFFILIATED WITH THE
INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

PROCEEDINGS.

SOME FEATURES OF THE PRESENT STEEL RAIL QUESTION.

Annual Address by the President.

The Bessemer steel rail has a marvelous history. The outgrowth of an attempt to make wrought iron cheaply, it came in just at the time when the wrought iron rail was beginning to demonstrate its unfitness to stand the pounding of the larger locomotives of the day. It perhaps is not too much to say that the Bessemer steel rail has made the modern railroad possible, and that without it, or its equivalent, the world's development would be half a century behind its present advanced position. And yet, notwithstanding all its merits, and its wonderful record in the past, no candid mind can view the present steel rail situation, and not be impressed with the thought, that the steel railroad rail, or, perhaps more comprehensively, the railroad track of to-day, is called upon to justify itself in the eyes of the public. No record of past success, however wonderful it may have been, will suffice to meet the present conditions. The changes in methods of transportation during the last quarter century, the increases in sizes of cars and locomotives, with the consequent increase in wheel loads, and in the strains produced, have made new demands upon railroad tracks, and especially upon the rail, as the most important element in the track. It is plain we think, that modifications at some point, and possibly at many points, are essential,

in order that the new conditions may be successfully met. The startling record of rail breakages which has characterized the past two or three years, the rapid wear, and the almost appalling deterioration, due to crushing and flattening, of rails in track, have produced an outcry against the steel rail, which, seconded by the technical press, has culminated during the past two years, in a charge of criminal negligence on the part of those engaged in the manufacture of this great essential of railroad operation. No one at all conversant with the situation can successfully maintain that the subject is not a pressing one, and I am sure that all will agree that there is necessity for calm, cool, dispassionate consideration of the various elements involved in the problem. Perhaps you will bear with me while I attempt to discuss some features of this most important and most interesting situation.

And first let us briefly consider the changed conditions. It is surely not too much to say that in the past twenty-five years the changes in the conditions in which the rail is involved, have in three respects been most noteworthy: First, the average speed of trains has been largely increased, second, the average wheel loads of cars have been increased seventy-five per cent. and of locomotives over one hundred per cent., and third, on some of the larger and more important railroads, the volume of traffic has increased at least 300 per cent., and perhaps more.

The influence of increased speed on the life of the rail, is not easy to estimate, nor indeed can much that is positive be said on this head. Undoubtedly some of the shocks which the rail must sustain, are increased in severity by higher speed. This we think, is clearly the case when a higher speed train passes over a loose joint. The end of the rail to which the wheel is approaching, must have a more severe blow with a fast train than is given by a slower moving one. On the other hand there seems to be some evidence that for those strains, whose severity is affected by the length of time the load is applied, the increase in speed may not be so injurious an element. We are all familiar with the fact that on thin ice, the boy who goes rapidly, is less liable to break through and get a ducking, than one who moves more slowly, and in exactly the same manner the quick stepper sinks less deeply into semi-plastic mud, than the one who goes with slower, measured tread.

The increase in wheel loads is a more important matter. Without going into the mathematics involved, and ignoring the apparently unsolved problem as to whether the rail under a moving load acts exactly like a beam supported at both ends and loaded in the middle, there is seemingly fairly good evidence that, all things else remaining the same, doubling the wheel load, approximately doubles the strain in the metal. It follows, therefore, when wheel loads are doubled, either that the rail section and supports in use at the time must have sufficient margin in them, to safely endure these doubled strains, or that some changes are essential.

The increase in traffic is also a very important matter. No point in the science of strength of materials, if we may use the expression, is apparently better established, than that repetition of stress, has a direct bearing on the life of the part so strained. There is much indication, and the evidence is rapidly increasing, that the life of a rail, not only as regards wear, but also as regards its freedom from disintegration and breakage, is a function of the number of wheel loads that pass over it. We are strongly inclined to the view, therefore, that we are on firm ground when we say that even though we grant that the rails made twenty to twenty-five years were good and gave satisfactory service, it is as clear as noon day, that the changed conditions of the present day, especially the heavier wheel loads, and the increase in traffic, demand changes in the practices that were prevalent at that time. No one, we are confident, will dispute this position, and I doubt not every railroad engineer within the sound of my voice, will say to himself, "we do not expect the old rail to be satisfactory under the new conditions; many changes have been made, and yet the rails are unsatisfactory."

Let us examine this matter a little, and ask ourselves the question plainly, what have the railroads done to meet the changed conditions? First and foremost, the weight of the rail per yard has been increased. As a matter of record it may not be amiss to mention the successive steps of this increase on one large railroad. Within my own memory and study of this subject, the following weights of rail have been employed: 56 pounds per yard, 60, 67, 70, 75, 85 and 100. The increase is quite noticeable, the latest form being nearly double the weight of that first used. The battle of the sections, or the form in which the increase of weight has been disposed, will be referred to later on.

Yet it may be said, "it is granted that the weight of rail has been increased, but what evidence is there that the increase has been sufficient?" "Is not a rail weighing 110, 120 or even 130 pounds per yard, essential to meet the strains produced by the changed conditions above referred to?" Upon this point it is possible to say that most careful studies have been made, using the best obtainable data, and making allowances for what is more or less unknown and uncertain, and that these studies indicate that the weight of rail to carry the increased wheel loads, has been increased more rapidly than the wheel loads, and that the actual strain with the heavier wheel loads, is no greater than was the case in the lighter rails under the lighter wheel loads formerly employed. It may be added that if 12,500 pounds per square inch is assumed as a safe working stress for such steel as rails are made of, the present 85- and 100-pound rails show stress well within this limit, even under a static wheel load of 30,000 pounds, with a dynamic augment of sixty per cent. of the static load. If these studies can be trusted, therefore, it would seem that so far as weight of rail is concerned, the railroads have done all that could reasonably be required to meet the changed conditions with which we are dealing.

But again it may fairly be asked, we think, whether increase in weight of rail is all that is required. No principle of structural construction is better established apparently, than that the support which material under strain receives is an essential element in its successful behavior. The rail alone cannot carry the load. It must be properly supported and perhaps, mind you, I say "perhaps," failure to properly support the rail may account for some of the rail failures, of which we have heard so much during the past few years. Good railroad track involves a properly drained sub-grade, ballast, ties and rail fastenings, splice bars, and other joint material, and the proper maintenance of these, as well as rails. In other words, the rails are only one of the elements in the problem; I grant you, the most important one, but still not the only one. It would be manifestly unfair to blame the rail for failures which may be clearly due to defective support or fastenings. What then have the railroads done during the past fifteen to twenty years, in the way of supporting the rail, to enable it to successfully meet the changed conditions we are discussing?

First, as to ties. It is well known that with the advent of the heavier rail, some railroads actually diminished the number of ties per rail. This may have been all right during the transition period, but it is manifest that the strain in the rail is increased by such practice. More recently, as the number of heavier cars and locomotives has increased, we believe the number of ties has been increased again, until now, in the best tracks, as many ties are used as can successfully be employed and leave room for tamping. It may not be amiss here to call attention to a characteristic of ties that bids fair to be of increased importance as time progresses. Owing to the continually diminishing supply of forest products, it seems fair to expect that the supporting face of the tie under the rail will gradually get less as the necessity for using smaller ties increases. This will clearly lead to an increase in strain in the rail, and may even now be an element in the problem. Perhaps the general use of a tie plate on wooden ties, or the development of an entirely successful steel tie, may obviate this difficulty before it becomes too serious.

The joint and the splice bar and fastenings have received an immense amount of study during the past twenty years, and while it would probably be too much to claim that the problem has been completely solved, no accusation that the railroads have been idle in this respect, can be successfully maintained.

The ballast holds the ties in place, distributes the strain produced by the load to the sub-grade beneath, and undoubtedly performs some function in drainage. Taken in general, it is undoubted that there has been gradual increase in ballast, as the wheel loads and traffic have increased. That it has reached the proper thickness yet, even on those tracks which are best maintained, we fancy no one would be willing to affirm. We believe there are engineers who think that twice or even three times the present standard thickness of ballast is requisite, in order that the load may be properly distributed over the supporting surface beneath.

The sub-grade is the foundation and as such is unquestionably one of the most important elements in the problem. No amount of money spent on rails, ties, splices and ballast, will give successful results, on an inferior sub-grade. Our belief is that the importance of the sub-grade has not been sufficiently appreciated by engineers

in the past. Indeed there are indications that rail failures are a question of geography. The same rail, with the same locomotives and cars, and the same density of traffic, will have far less failures, if the sub-grade is sandy, porous, and well drained, than if the sub-grade is dense, heavy clay, which tenaciously holds water and for quite a portion of the year, may be called a more or less modified mud hole. The great enemy of the trackman is water, and our firm belief is that if more study had been put on the problem of keeping water out of the sub-grade, fewer rail failures would have characterized the past.

The maintenance of track, that is the amount of money and labor put upon the track to keep it up to its work, tamping ties, screwing-up track bolts, keeping up renewals, giving heed to drainage, etc., has necessarily increased as the wheel loads have become heavier and the density of traffic has increased. I know of no evidence that indicates that there has been a blameworthy falling off in any of these respects with the advent of the heavier rail. It is undoubtedly true that track expenses, even with the same maintenance of line and surface, show a tendency to diminish, the heavier the rail. It is easy to see why this should be so, since the heavier and stiffer the beam, the more the strains produced by the moving load are distributed, and consequently the less need that money should be spent to cause each minute part of the support to do its maximum duty.

And this brings us to an exceedingly interesting phase of the rail problem. During the past year or two, in discussions and committee meetings over rail specifications, the desire on the part of some railroad engineers, has occasionally cropped out, to have a rail of minimum weight per yard, and yet so good that even though the track might not always in every detail be kept up to standard, there would still be no failures. According to their view if the rails were what they should be, inferior track maintenance would be a very small matter. And, singularly enough, it seemed to be a little difficult for these engineers to see that this unloading of the whole problem upon the steel rail manufacturers, was not entirely legitimate and praiseworthy. We hope to pay our respects to the steel rail manufacturers a little later, but we are compelled to say that this view of the case does not commend itself to us, and that we do not think responsibility can be so easily

shifted to other shoulders. No one can be more desirous of good rails than we are, but failures in track maintenances cannot, we think, fairly be put upon the rail. On the other hand, we do think there is a legitimate question connected with this phase of our subject. This is, the requisite safety being always maintained, where does true economy lie in this contest between the rail and the track maintenance? There are three possibilities.

(1) Would better rails with the same weights per yard as are now in use, even though obtained at increased cost, result in such diminution of track expenses, that economy would result?

(2) Would the same so-called inferior grades of steel that are now being furnished, with increased weights per yard, it being conceded that increased weights per yard are not essential from the standpoint of strains, result in such diminution of track expenses, that economy would follow after paying for the increased weights? and

(3) Would better steel and increased weights per yard, at even still greater cost, be followed by such saving in other track expenses, that it would be true economy to obtain such rails?

We do not believe the data exist at the present time, that would enable a satisfactory answer to be given to any one of these three questions. And yet they seem to us to open up an extremely important field. It costs a certain amount each year to maintain a mile of track, including rail renewals, and we have never seen any figures that show that the distribution of the expense to each item is such that the sum total is a minimum. Perhaps better rails, perhaps heavier rails, perhaps the two combined, even at greater cost for rails, would bring about this desired result.

Returning now to our query as to what the railroads have done to meet the changed conditions produced during the last twenty years or more, by heavier wheel loads, and increase of traffic, it is difficult to see how any fair-minded person can think that the railroads, taken generally and as a whole, have not made a sincere effort to meet these changed conditions. It is granted that the action taken may not always have been the wisest action, that failures and mistakes have been made, and, indeed, it is undoubted that at times and places, more vigorous action should have been taken, and that too frequently there has been a disposition to lay the whole burden upon the steel maker, and yet, notwithstanding

all this, it would, we think, be hard to maintain that there has not been much sincere, conscientious and faithful effort on the part of the railroads to meet the problems due to changed conditions, with which they have been confronted.

Let us turn now to the record of the steel rail manufacturer in this matter, and ask ourselves calmly and dispassionately, what has he done during the past fifteen or twenty years to assist in meeting the changed conditions, about which so much has already been said? Has he made better steel? Has he spent time and money and brain power in faithfully trying to improve his product? Has he heartily and earnestly coöperated with the railroad engineer, in his efforts to secure safe transportation of persons and property?

Before trying to answer these questions let us spend a moment with this problem of responsibility. It has already been hinted several times that there are railroad engineers who would be quite willing to put the whole burden of rail failures on the steel makers, and, on the other hand, it is quite as certain that the steel makers seem to have been not only willing, but as a matter of fact have again and again washed their own hands entirely of responsibility in this matter, and claimed that it was simply a question for the inspectors and engineers. We have heard them say in effect, not once, but many times, "Our mills are open to you. Here are the rails. Take them or leave them," thus practically putting the whole responsibility of accepting and using defective and inferior material upon those who from the nature of the case cannot know as well as the manufacturer, which are good and which are bad, and who many times have no alternative but to take what they can get. Not so do we understand the relation between the producer and consumer. From the nature of the case, the producer has essential information, that the consumer does not have, and on the other hand, the consumer has essential information that the producer does not have, and it is only by working together conscientiously, honestly and harmoniously, that the best results can be obtained. We do not understand that the manufacturer of any material for the use of a public service corporation, where safety to human life is involved, can properly assume an air of indifference, can hold back essential information, which he as the maker of the material has, and the consumer has not, or can

justly fail to coöperate in every reasonable way to the end that only safe material goes into the tracks.

The making of steel rails for use under high-speed passenger trains, is something more than a mere commercial proposition. Both the producer and the consumer have great responsibilities in this matter, and neither can lay them aside or shift them upon the other.

Coming back now to our query as to the behavior of the steel rail manufacturers during the past fifteen to twenty years, we are frank to say that with the most sincere desire to be strictly fair in this matter, we cannot regard this behavior as eminently praiseworthy. There has been far too much antagonism, far too much holding back essential information, and far too great a failure to coöperate. Many within the sound of my voice will bear me witness that in conferences over steel rail specifications, it has many times been simply impossible to get such information from the representatives of the steel makers, as would enable critical points in the specifications to be wisely decided. And this is not the worst of the matter. Within two years past, entirely reputable engineers of railroad companies have said in my presence that they had been told by rail manufacturers, that if they did not take the rails offered, irrespective of specifications and tests, they would not get any. Still further, if I may judge from my own experience, it is only within the past year, or perhaps year and a half, that it has been possible to have what might fairly be called a consultation with the experts of the steel rail makers, over any point that involved the quality of the product, in any conciliatory, coöperating way. Far too much, we fear, the manufacture of steel rails has been studied in the past simply from the commercial standpoint.

And how about the steel itself? Is the steel that goes into rails to-day better or worse than that made fifteen or twenty years ago? It is well known that there is a general belief among railroad engineers, that the steel in the rails to-day is nothing like as good as it was twenty or twenty-five years ago. We are not quite able to follow these critics to the extent that they want to go on this point. Our judgment is that in two respects there is reason to think the steel in Bessemer rails is inferior to that made twenty-five years ago:

(1) The larger ingots of the present day necessarily lead to increased segregation, and in so far as segregation is a serious element in the quality of steel, there is little room for doubt that the steel in the rails of to-day is inferior to that made some years ago. We think it fair to say that there is need for further study as to how far segregation, apart from internal physical defects with which it is so often associated, is a menace. We cannot help wishing that it was not in the rail to anything like as serious an extent as many analyses show it to be, but at the same time, if the ingot was thoroughly sound throughout, we should like to study segregation farther before feeling willing to say the final word.

(2) In so far as the more rapid working of the Bessemer process which has seemed to characterize its full commercial development during the past twenty years or more, has led to incomplete action between the final additions and the blown metal, and to higher finishing temperatures in the finished rail, the metal must be inferior. The making of steel is a chemical process, and every chemist knows that all chemical reactions require time, and it is to be feared that Bessemer metal is, many times, cast in the ingot mold, before the reactions are complete. The effect of this is certainly bad. Sound ingots cannot be obtained in this way, and if the ingot is unsound, good rails cannot be made. Again we are quite well aware that high finishing temperatures are blamed on the rail section. Thin flanges, by getting cool too soon, seem to make it impossible to roll the head at the proper finishing temperature, and it is to be confessed, that in as far as the section is responsible for too high finishing temperature, the steel maker cannot fairly be blamed. But for all other causes leading to too high finishing temperatures, it is difficult to see how the responsibility is to be shifted. Notwithstanding these, as we regard them, just criticisms of the later made Bessemer steel, it is undoubted that a large percentage of the output of rails of the last twenty years has been approximately as good as that previously made. We are inclined to think that those who so vigorously denounce the later made steel, forget the enormously increased traffic which these rails have carried. The lack of reliable figures showing comparative tonnage of the earlier and later made steel is a very serious handicap in reaching a satisfactory conclusion in this matter.

But whether the earlier or later made steel is better or not, it is certain that the need of to-day is for a steel even better than that earlier made. The speed is greater, the wheel loads are greater, and the tonnage is greater, and the steel, to successfully meet these changed conditions, should also be better. Do we find then that during the last twenty years, there has been real conscientious study on the part of the Bessemer steel maker, as to how to make better rails? Are the ingots any less unsound, and more free from shrinkage cavities, pipes, gas bubbles and blow holes? Have any of the suggested means of overcoming or minimizing segregation, and diminishing the pipe been tried on Bessemer ingots? Have any of the Bessemer steel rail manufacturers or their experts contributed something toward the solution of the problem of how to secure safer and better rails? Have they welcomed and seemed willing to fairly try suggestions that might possibly lead to improvements in their products? Have they willingly accepted more frequent testing, to the end that inferior material might be kept out of the tracks? In so far as they have done honest conscientious work in any of these directions, let all honor be given them. In so far as they have failed, let us not add to their shame by discussing this phase of our subject farther.

Let us turn now to two or three other features of our subject; and first, to the question of sections. It has already been hinted that the section has been blamed for some of the difficulties encountered in making good rails. The makers have urged, and we think with good show of reason, that the distribution of metal between the head, web and foot of the rail, in many of the sections, was such that it was practically impossible to finish the heads at the proper temperature. And since the sections were specified by the railroad engineers, the rail makers have felt, and we think justly so, that in so far as the section was an element in good rails, they could not be held responsible for poor rails. This criticism was felt to be so valid that within the past two years, two independent organizations have taken up the question of section de novo. One large railroad, through a committee of its own officers, reinforced by practical rail manufacturers, and the American Railway Association, through one of its own committees, assisted also by a number of practical rail makers, have both, but independently,

devoted much study to the section. The results of their deliberations have culminated in three types of sections. The one known as Section "A" of the American Railway Association, is characterized by a shallower head, wider base, with thin flanges, and a greater height of section than either of the other two. Regarded as a beam or girder, this section is undoubtedly the strongest section of the three. It is apparently advocated by those who think that more of the duty of the track should be borne by the rail, and less by the other elements. It is obvious, as has already been stated, that the stronger the rail, as a beam or girder, the more the strains are distributed, and the less need, therefore, for exacting attention to the other features of track maintenance. Its advocates think that the distribution of metal between head, web and foot, is such that the rolling difficulties and especially the question of finishing temperatures, can be met with entire success. It would be a bold man who would be willing to affirm that this section will not ultimately prove to be the best of those under consideration, and especially that the transference of more of the duty to the rail, will not result as has already been indicated, in ultimate track economy. Those who oppose this section, fear that the shallow head is an element of weakness. According to their view, with such steel as it is at present possible to get in rails, the pounding of the heavy traffic will lead to such crushing and splitting of the heads, owing to internal physical defects in the metal, that the section will prove a failure, especially on roads with heavy wheel loads and dense traffic.

The second type of section, known as Section "B" of the American Railway Association, is modified to meet this latter view. The head is practically the head of the American Society of Civil Engineers' section, thicker flanges, less height over all, and the same thickness of web. The distribution of metal is believed as in the "A" section, to successfully meet the manufacturers' criticism, the head and foot having slightly over forty per cent. each of the metal, and the web the balance. This section is confessedly a compromise, but there goes with it the hope that it will ultimately prove itself worthy of being adopted as the standard recommended section of the Association. This section is weaker as a girder than the "A" section, but is believed to have sufficient factor of safety so that no difficulties will arise from this cause.

These two sections have been proposed as "recommended practice" by the American Railway Association, and the question as to which is the better of the two, or indeed as to whether some other section is not still better than either, has been referred to the American Engineering and Maintenance of Way Association, an organization composed largely of railroad operating officers, to study and accumulate data, and make a report, after the sections have been tried in actual service.

The third section, known as the "P. S. Section" of the Pennsylvania System, is a step farther away from the "A" section. It has a still heavier head, a narrower base, thicker flanges, and the same thickness of web, as the "B" section. The radius of the web is smaller, thus producing more of a buttress where the head and web join. The experience of the Pennsylvania System seems to be that with their heavy wheel loads and dense traffic, and with the grade of steel that it is now possible to get in rails, more rails fail from crushing and disintegration of the head, apparently due to the pounding of the traffic, than from any other one cause, and accordingly in this section, the maximum effort has been made to strengthen the head in its weakest point. The distribution of the metal is satisfactory, and the strength of the rail as a girder or beam, is practically the same as the "A" section, and is believed to be abundant for present conditions.

Which if any of these three sections will survive and become the standard section of the railway world in this country, it is of course impossible at the present moment to say. However, the issue is now clearly defined as never before, and the different ideas prevailing among expert engineers are clearly represented in the types presented. All the sections have been designed to meet the criticism of the steel makers, so that from this time forward, if these sections are used, there should be no excuse for improper finishing temperatures. It may happen that progress in steel metallurgy will so diminish physical defects in the steel, and so minimize the adverse effect of segregation, that all three sections will be found worthy of perpetuation. It is believed that a decided step forward has been taken by the preparation of these three sections, and much is expected from them in the next few years to come.

The question of the discard has occasioned many words,

during the discussions of the past two years, and we had almost said "bitter" words. There is material enough in this feature of our subject for a long paper. I fancy many of us had more positive ideas on discard three or four years ago, than we have at the present moment. The more the subject is studied the more difficult it becomes, seemingly, to express a final opinion that we are willing to stand by. If every ingot solidified in the typical way, and was like every other ingot, the question of discard would be easy. But it would almost seem as though every heat of steel was a law unto itself, and was different from every other heat of steel, and nearly the same might be said of every ingot. When we come to consider the conditions there seems to be much reason why this should be so. The differences in temperature at the time of casting, the more or less incompleteness of the chemical reactions, when the metal leaves the ladle, the differences in chemical composition of the different heats, the rate of pouring, the differences in the condition of the molds, the differences in rate of cooling dependent on surrounding conditions, the differences in practice at the different works, especially in the matter of covering and artificially cooling the top of the ingot, the length of time that elapses before the ingots are stripped, and the more or less fluid condition of the metal on the inside of the ingot when it reaches the first pass, to say nothing of accidents or mishaps that may occur in handling the semi-fluid ingots, all have an influence on the location of that part of the ingot which is supposed to contain the poorest of the metal, and which it is the object of the discard to prevent from getting into the rails.

In view of these uncertainties, we cannot but think that the position taken by the Pennsylvania System and the American Railway Association in their proposed specifications, is the most reasonable one, viz., to leave the discard to the manufacturers, and to safeguard the product by proper tests, especially by choosing the test piece from such a location, and making the rejections such, that it will be to the interest of the manufacturer to voluntarily discard the metal which will not stand test. This has been our practice at Altoona for many years. We have a number of specifications for different steel products, in which the word "discard" does not occur, and never has. An illustration will perhaps make this whole matter clear. When our axle specification was issued

the whole question of discard, was most carefully considered, and it was finally decided to select one axle at random for test from each heat of steel, after the axles from that heat had all been made up, stamped with the heat number, and put in a pile by themselves, and further if the test axle stood all the tests, all the axles made from that heat were accepted, and if not all were rejected. After the specification had been in operation a short time, a manufacturer who had worked with us during the development of the specification, said to me, "There is steel enough in each one of our ingots to make thirteen axles of the size you are now using. As a matter of fact we only make and offer you for test nine, for if we should make the whole ingot into axles, and you should get for test one made from the steel which we now discard, you would condemn the whole heat, quite to our loss." We are firmly of the opinion that the matter of testing rails can be so handled, as to give similar satisfactory results.

And this brings us to the question of tests and testing. During the past few years, much light has been thrown on this subject, and the truth compels us to say, that a situation has been found that, in some respects, would be ludicrous if it was not so near the tragic. Let us see what the conditions have been:

(1) The manufacturers have, in many cases at least, selected the rail end as sample for test. The specifications being silent on the selection of the test piece, they naturally have urged that there was nothing to prevent their doing this, and they naturally again have, so far as information can be obtained, chosen the best steel in the ingot for test. It is not claimed that all specifications have been so loosely drawn, as to permit such a suicidal practice, but it is certain that some of them have, and that the practice has been in vogue.

(2) The best two-in-three principle has pervaded many specifications, that is to say, if the first rail end stood test, the heat was accepted, but if it failed, a second was tested. If this likewise failed, the heat was condemned. If, on the other hand, it stood test, a third was tested, and the fate of the heat was decided by the majority. It would almost seem as though the specification had been drawn, not with the idea of being sure that only good rails should be accepted, but with the idea of being sure that as many heats as possible should be accepted.

(3) Only one heat in five was tested, that is to say, as we understand the matter, if the rail end stood test, and the heat was accepted, that acceptance carried four other heats with it. But singularly enough, if, on the other hand, the heat was rejected, that rejection only covered the heat from which the test rail end came, and the four preceding or following heats, as the case might be, got another chance for their lives. The unsatisfactoriness of such a method of testing, it seems to us, must be evident to every candid mind that knows anything about the making of steel. As has already been stated, every heat of steel is a law unto itself, and there is no certainty, that because one heat or blow is good, the preceding or succeeding four are equally good, any more than there is a reasonable presumption that if one heat is bad, the preceding or succeeding four are likewise bad. And while it is agreed that when everything is working well, successive blows from the Bessemer converter may be similar in many respects, it is not agreed that testing one blow in five gives any reasonable assurance that only good rails are accepted for use in the tracks.

We fancy the rather loose testing described in the three items above, started in the earlier days, when the strain on the rail was far less than at present, and the traffic far less dense, and has been perpetuated, partly owing to inertia on the part of railroad engineers, and partly owing to the resistance of the rail manufacturers. The wonder is, with such loose, log-rolling testing as has been in vogue, not that there have been so many rail failures, but that there have not been more.

But this is not the whole story. Until quite recently the specifications have been equally loose in regard to the drop testing machine employed in making the tests. Weight of tup has been carefully stated, and the height from which it must fall has been given; indeed in some cases the foot pounds of the blow are carefully given for each weight of rail, but it has apparently been forgotten that the anvil or support on which the rail rests when it receives the blow is a most important element in the problem. It has recently developed that at one steel works the anvil was a couple of ingots laid down side by side, with appliances for holding them in place, and supporting the test rail, the whole resting on boards placed on the ground. At another steel works, the anvil weighed 3,000 pounds, and rested on boards, stones and gravel.

One rail manufacturer recently said in my hearing, "As long as the railroads did not object, why should we take measures to increase the severity of the test by putting in heavier or better supported anvils." It is gratifying to be able to state that some of the more recent specifications, while according to our ideas still far from satisfactory in the matter of testing, do show marked improvement in some of the respects mentioned above, and still better, that the rail manufacturers are co-operating in and actually suggesting some of the improvements.

A few words now in regard to some of the details of testing rails; and first, in regard to the drop test. It is well known that many testing engineers do not favor the drop test for rails. To our minds, however, it is the only possible available one for the present, and the following considerations seem to us to have weight in confirming this view:

(1) It tests the whole rail in the condition in which it goes into the track, instead of a small fraction of the rail, as is requisite in all cases of prepared test pieces.

(2) It is sufficiently rapid, so that even though every blow is tested there is no fear of delaying the output of the mills, while waiting for test pieces to be prepared, or for slower tests to be made. We have known of a case where, with sufficient force to handle the test samples, fifty-five tests have been made in half an hour, on a modern drop-testing machine.

(3) There seems little doubt but that some of the strains or shocks which the rail actually receives in track, are similar to those produced by the drop-testing machine. This is, we think, clearly the case with a loose joint and a rapidly moving train. In case the track bolts have become loosened, the end of the rail, when the approaching wheel mounts it, certainly gets a blow similar to that given by the drop-testing machine. We have known rails which have given long service in track, to be broken in this way, and the fracture showed perfectly clean, sound metal.

(4) Finally, if the specification requires that the deflection be taken, the drop test reveals a good deal in regard to the physical properties of the steel.

Second, in regard to the selection of the test piece. We fancy it goes almost without saying, that this should always be made by the inspector. In regard to location of test piece, it is of course

understood that in shearing the ingot into rail blooms, it is necessary to make the bloom from which the test rail end is to be taken a little longer than the others. It is therefore essential that the inspector or the specification should designate the bloom from which the test will be taken. Some recent specifications, wisely we think, designate the top bloom of the ingot for this purpose, it being generally understood that the so-called "pipe," if there be any, and the greatest segregation and physical defects, will be in this bloom. We may, perhaps wisely, call attention to the fact that if a cover and cooling devices are used on the top of the ingot when it is cast, the poorest steel in the ingot will not be near the top end of the top bloom, but more probably near the bottom end of the top bloom, so that if the inspector takes his test piece from the top end of the rail made from the top bloom, he will be more apt to deceive himself, than if he has the test piece cut from the bottom end of the top bloom. We have sometimes thought, when inspecting the practices of casting and cooling ingots in certain works, that it would be better to take the discard between the first and second blooms.

Third, in regard to height of drop. We have always opposed extreme severity of tests. Owing to the defects in the anvil previously referred to, if indeed they have been general, it is apparent that but little information that is of value, and that is safe to follow, can be obtained from previous tests. Our own view is that something a little more severe than the rail will receive in actual service, enough for a reasonable factor of safety, is all sufficient. The trouble is we do not know how severe the shocks in service are. Some recent tests of rails which had broken short off in track, made by the Research Committee of the Pennsylvania System, seem to indicate that a fifteen-foot drop with a 2,000-pound tup, and a 20,000-pound anvil, would have rejected two-thirds of those which failed in service; also, that the fifteen-foot drop actually broke as many test pieces as the nineteen-foot drop, other conditions being the same. These tests should be much amplified before final conclusions can be reached, but as far as they go, they seem to indicate that we must look to other causes than defective or poor steel for a portion of the rail breakages, and that extremely severe testing is not necessary.

We have taken so much time with what precedes, that there is

no opportunity to discuss a number of other features of the rail situation. The subject is far too large for a single paper. It would be interesting to consider quite at length, how to manage the results of tests, in such a way as to be strictly fair to the manufacturers, and at the same time, prevent the acceptance of inferior or defective rails. We would suggest that those especially interested study the specifications of the Pennsylvania System recently issued, and those of the American Railway Association, proposed at its recent meeting in New York City, April 22, 1908. Much might be written on the chemistry of the steel rail, and especially the phosphorus limit; also on open hearth steel rails, but time and space forbid. The wear of rails has not been touched at all, and must be deferred to another occasion.

Summing up and putting in concrete form our views on the present situation, we are inclined to say:

(1) The crying need of the hour, is positive, definite information. Upon dozens of points, no positive data exist. During the discussions of the past two years or more, opinions have been as plentiful as leaves in autumn, but of positive, reliable, statistical information, or figures taken from properly kept records, there has been a dearth that was fairly oppressive. The steel makers have not been as deficient in this respect as the railroad engineers.

(2) The time seems opportune for genuine progress to be made. The railroads through their organization, which in a sense speaks for them all, the American Railway Association, have taken hold of the matter with vigor, and have developed a large amount of valuable information, and for the first time in my twenty-five years' study of this subject the steel rail manufacturers have shown a less antagonistic and more conciliatory and co-operative spirit than has usually characterized them.

(3) The specifications proposed by the Pennsylvania Railroad System, and by the American Railway Association, seem to us to represent genuine progress, and to be worthy of most careful study and trial. While they may be said to represent perhaps the best that can be done, until more positive knowledge is obtained, he would be a bold man that would claim that they will ultimately be satisfactory or final.

(4) Whether the Bessemer process can be so modified and improved as to enable it to furnish rails that will be entirely satis-

factory under the heavier wheel loads and denser traffic of to-day, and the near future, or whether the basic open hearth will soon be the source from which steel for these rails will be furnished, are questions worthy of serious attention. Our own feeling is that if a small fraction of the time and money that has been spent in the past over the commercial development of the Bessemer process, shall, in the next few years, be spent in getting sound ingots, free from blow holes, slag and manganese sulphide, if this shall be found to be as serious as it now looks, and in overcoming or minimizing segregation, it will last for many years to come.

(5) The American Society for Testing Materials, has a most important duty to perform at this juncture. By stimulating the development of information, by furnishing an arena for the presentation of such papers on the metallurgy of steel as are on its program for this meeting, by arousing interest in testing machines and methods of testing, by furnishing a forum where producer and consumer can meet on common ground and discuss their differences unhampered by commercial considerations or by artificial distinctions of professional ethics, and by keeping their own specifications up to date and utilizing new information as fast as it is obtained, it can so fill the field which it occupies, that when the ultimate record is made up, its contribution will be by no means among the least.

We beg not to be misunderstood in this matter. If we have called attention to loose practices, if we have indicated that as we look at the matter, rail manufacturers have in the past been a little too much actuated by commercial considerations, and have too vigorously antagonized the efforts made by inspectors and engineers, to secure better and safer rails for the track, if we have pointed out that in some cases specifications have been loosely drawn, and that tests and inspection have too often been one-sided and inefficient, it has not been for the purpose of holding any one up to ridicule, or to make statements that would appeal to the popular fancy, but with a sincere desire to have the mistakes and shortcomings of the past so well understood, that they will not be perpetuated in the future. There is much indication, and it may well be, that what has been done in the past was sufficient for its day. But our sincere belief is that past practices are no longer applicable, and our plea is for more conciliatory and more con-

scientious action on the part of the rail manufacturers, and for more thorough and exhaustive study of all the elements of safe economical track on the part of railroad engineers, to the end that the changed conditions about which so much has been said may be successfully met, and the rail transportation interests of the country be put on a safer and better basis.

REPORT OF COMMITTEE A ON STANDARD SPECIFICATIONS FOR IRON AND STEEL.

At the last annual meeting Committee A recommended the adoption of revised Standard Specifications for Steel Rails embodying amendments proposed at the preceding Annual Meeting, and certain additional changes. These proposed revised Standard Specifications for Steel Rails were referred, by two-thirds vote, without change, to letter-ballot of the Society. The letter-ballot, canvassed August 26, resulted in the adoption of these specifications by an affirmative vote of 125 against a negative vote of 18.

At a meeting of the Executive Committee, held on October 4, the Secretary was instructed to append the following notice to the report of Committee A, on Standard Specifications of Steel Rails, in Volume VII of the Proceedings:

"The specifications embodied in the above report were referred to letter-ballot of the Society by two-thirds vote at the Tenth Annual Meeting, and adopted by a vote of 125 affirmative and 18 negative, canvassed August 26.

"The standard length of finished rails was originally fixed by the Committee at 30 feet instead of 33 feet, on account of the then existing scarcity of cars, especially in the East, of suitable length for the shipment of longer rails. This condition having disappeared the Executive Committee, acting on the recommendation to that effect from Committee A, and confirmed by the members of that Committee by letter-ballot without a dissenting vote, has authorized the adoption of the 33-foot length in the Standard Specifications, since the change is one not affecting the quality of the product but dictated by practical considerations only; this action to be subject to the approval of the Society at the next annual meeting.*

"The Standard Specifications embodying this change, and the corresponding modifications in paragraph 4, on finishing tem-

* This action was approved.—Ed.

perature, and in paragraph 9, on shorter acceptable lengths, follow in the succeeding pages."

At a meeting of the Executive Committee, held on January 6, the Secretary was instructed to communicate with the Chairman of Committee A, with a view of calling his attention to the desirability, in the judgment of the Executive Committee, of considering the revision of the Standard Specifications for Steel Rails.

At a meeting of Committee A, held at Columbia University, January 28, 1908, the following modifications were adopted with the understanding that they would be referred to letter-ballot of the Committee:

(1) To modify paragraph 3 to the effect that one drop test shall be made on every blow of steel for rails weighing 85 to, and including, 100 pounds per yard.

(2) To modify paragraph 14 to the effect that No. 2 rails shall be accepted to at least 5 per cent. of the whole order.

(3) To add the following sentence to paragraph 14: Rails rejected under the drop test will not be accepted as No. 2 rails.

The letter-ballot of the Committee resulted in the following vote: (1) affirmative 23, negative 7; (2) affirmative 23, negative 7; (3) affirmative 17, negative 13.

At the meeting above referred to the question of modifying the specifications as to "discard" and "heights of drop" was referred to a sub-committee of five members.

At a meeting of Committee A, held at the Engineers' Club of Philadelphia, April 27, the sub-committee on discard and heights of drop, consisting of Messrs. C. B. Dudley, Chairman, W. A. Bostwick, E. F. Kenny, J. P. Snow and W. R. Webster, reported as follows:

The satisfactory height of drop is affected by the rail section used, and very greatly by the drop-testing machine employed. The whole subject of rail sections and drop-testing machines is at the present moment in a very chaotic condition and much experimental work is now being done. Furthermore, the relation between the strain in the rail produced by the drop test, and the strains produced in service, has not yet been ascertained with any degree of certainty. The information thus far obtained, indicates:

First, that many of the drop-testing machines used in the past, have given very fallacious results, owing to inferior foundations and anvils

Second, that due to this defect in the drop-testing machines heretofore employed, but little reliable information in regard to the best and proper heights of drop can be obtained from past practice. Rapid progress is being made in the design and manufacture of drop-testing machines, and already a proposed standard design has been prepared, and it is hoped it will be in shape to be presented at the next meeting of the Society.

In view of these uncertainties, and until more positive data can be obtained by experiment, your sub-committee does not see its way clear to recommend any change at the present time, in the standard heights of drop of the rail specifications.

In regard to discard, the developments of the past few months indicates a disposition to relegate the amount of discard to the manufacturers, the consumers protecting themselves by such testing as, it is believed, will enable them to secure only good rails for the track. Whether this change in specifications, in the matter of discard, will result as favorably as its advocates hope, is still an unsolved problem. Experimental rails made under the changed specifications, have been put in track, and their behavior is being watched, but no one can yet foretell the result.

In view of this situation, and especially in view of the energetic discussion now going on between the producers and consumers, involving not only the manufacture and quality of the steel used in making rails, but also all the conditions of the service to which the rails are exposed, your sub-committee is inclined to think that the present wording of the Society's rail specifications, in the matter of discard, which is, that discard shall be agreed upon in each case by the parties in interest, is the best that can now be adopted.

On motion it was decided to continue the sub-committee for the further consideration of these questions, and to authorize them to coöperate, so far as practicable, with sub-committees of committees on rail specifications of other societies, with a view of bringing about a satisfactory basis of agreement.

The proposed amendments, referred to above, are as follows:

Paragraph 3. Add the following sentence after the first sentence in the paragraph: For rails weighing 85 to, and including, 100 pounds per yard, one drop test shall be made from every blow of steel.

Paragraph 14, first line, change the sentence: "No. 2 rails

will be accepted up to ten (10) per cent. of the whole order," to "No. 2 rails shall be accepted to at least five (5) per cent. of the whole order."

Add the following sentence at the close of the paragraph: Rails rejected under the drop test will not be accepted as No. 2 rails.

Respectfully submitted on behalf of the Committee:

W. R. Webster,
Chairman.

Edgar Marburg,
Secretary.

Note.—The above amendments were adopted by letter-ballot on August 15, 1908, and the Specifications as amended follow this report.

[For Discussion of this report, see page 109.]

AMERICAN SOCIETY FOR TESTING MATERIALS

PHILADELPHIA, PA., U. S. A.

AFFILIATED WITH THE

INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

STANDARD SPECIFICATIONS FOR STEEL RAILS.

ADOPTED AUGUST 15, 1908.

Process of Manufacture.

(1) (*a*) The entire process of manufacture and testing shall be in accordance with the best current practice, and special care shall be taken to conform to the following instructions:

(*b*) Ingots shall be kept in a vertical position in the pit heating furnaces until ready to be rolled or until the metal in the interior has time to solidify.

(*c*) No bled ingots shall be used.

(*d*) There shall be sheared from the end of the blooms formed from the top of the ingots not less than x* per cent., and if, from any cause, the steel does not then appear to be solid, the shearing shall continue until it does.

Chemical Composition

(2) Rails of the various weights per yard specified below shall conform to the following limits in chemical composition:

	50 to 59 Pounds. Per cent.	60 to 69 Pounds. Per cent.	70 to 79 Pounds. Per cent.	80 to 89 Pounds. Per cent.	90 to 100 Pounds. Per cent.
Carbon....................	0.35-0.45	0.38-0.48	0.40-0.50	0.43-0.53	0.45-0.55
Phosphorus, shall not exceed	0.10	0.10	0.10	0.10	0.10
Silicon, shall not exceed.....	0.20	0.20	0.20	0.20	0 20
Manganese	0.70-1.00	0.70-1.00	0.75-1.05	0.80-1.10	0.80-1.10

* The percentage of minimum discard in any case to be subject to agreement and it should be recognized that the higher this percentage the greater will be the cost.

(3) One drop test shall be made on a piece of rail not less than four feet and not more than six feet long, selected from every fifth blow of steel. For rails weighing 85 to, and including, 100 pounds per yard, one drop test shall be made from every blow of steel. The test shall be taken from the top of the ingot. The rail shall be placed head upwards on the supports, and the various sections shall be subjected to the following impact tests under a free falling weight: **Drop Test.**

	Weight of Rail. Pounds per Yard.	Height of Drop. Feet.
	45 to and including 55	15
More than	55 " 65	16
More than	65 " 75	17
More than	75 " 85	18
More than	85 " 100	19

If any rail breaks when subject to the drop test, two additional tests, taken from the top of the ingot, will be made of other rails from the same blow of steel, and if either of these latter tests fail, all the rails of the blow which they represent will be rejected, but if both of these additional test pieces meet the requirements, all the rails of the blow which they represent will be accepted.

(4) The number of passes and speed of train shall be so regulated that on leaving the rolls at the final pass the temperature of the rail will not exceed that which requires a shrinkage allowance at the hot-saws, for a 33-foot rail of 100 lb. section, of $7\frac{5}{16}$ inches, and $\frac{1}{16}$ inch less for each 5 lb. decrease of section. These allowances to be decreased at the rate of .01 inch for each second of time elapsed between the rail leaving the finishing rolls and being sawn. No artificial means of cooling the rails shall be used between the finishing pass and the hot-saws. **Finishing Temperature.**

(5) The drop testing machine shall have a tup of two thousand (2,000) pounds weight, the striking face of which shall have a radius of not more than five (5) inches, and the test rail shall be placed head upwards on solid supports three (3) feet apart. The anvil block shall weigh at least twenty thousand (20,000) pounds, and the supports shall be part of, or firmly secured to, the anvil. The report of the drop test shall state the atmospheric temperature at the time the test was made. **Drop Testing Machine.**

Chemical Analyses. (6) The manufacturer shall furnish the inspector, daily, with carbon determinations for each blow, and a complete chemical analysis every twenty-four hours, representing the average of the other elements contained in the steel, for each day and night turn. These analyses shall be made on drillings taken from a small test ingot.

Weight Section. (7) Unless otherwise specified, the section of rail shall be the American Standard, recommended by the American Society of Civil Engineers, and shall conform, as accurately as possible, to the templet furnished by the railroad company, consistent with paragraph (8) relative to specified weight. A variation in height of one-sixty-fourth ($\frac{1}{64}$) of an inch less, or one thirty-second ($\frac{1}{32}$) of an inch greater than the specified height, and one-sixteenth ($\frac{1}{16}$) inch in width will be permitted.

(8) The weight of the rails will be maintained as nearly as possible, after complying with paragraph (7) to that specified in contract. A variation of one-half ($\frac{1}{2}$) of one per cent. for an entire order will be allowed. Rails shall be accepted and paid for according to actual weights.

Length. (9) The standard length of rails shall be thirty-three (33) feet. Ten per cent. of the entire order will be accepted in shorter lengths, varying by even feet to twenty-seven (27) feet, and all No. 1 rails less than 33 feet shall be painted green on the end. A variation of one-fourth of an inch in length from that specified will be allowed.

(10) Circular holes for splice bars shall be drilled in accordance with the specifications of the purchaser. The holes shall accurately conform to the drawing and dimensions furnished in every respect, and must be free from burrs.

Finish. (11) *Straightening.*—Care must be taken in hot-straightening the rails, and it must result in their being left in such a condition that they shall not vary throughout their entire length more than 5 inches from a straight line in any direction, when delivered to the cold-straightening presses. Those which vary beyond that amount, or have short kinks, shall be classed as second-quality rails and be so stamped.

The distance between supports of rails in the gagging press shall be not less than 42 inches.

Rails shall be straight in line and surface when finished—the

straightening being done while cold—smooth on head, sawed square at ends, variation to be not more than $\frac{1}{32}$ inch, and, prior to shipment, shall have the burr occasioned by the saw cutting removed, and the ends made clean. No. 1 rails shall be free from injurious defects and flaws of all kinds.

(12) The name of the maker, the weight of rail and the month and year of manufacture shall be rolled in raised letters on the side of the web, and the number of blow shall be plainly stamped on each rail where it will not subsequently be covered by the splice bars. **Branding.**

(13) The inspector representing the purchaser shall have free entry to the works of the manufacturer at all times when the contract is being filled, and shall have all reasonable facilities afforded him by the manufacturer to satisfy him that the finished material is furnished in accordance with the terms of these specifications. All tests and inspections shall be made at the place of manufacture prior to shipment. **Inspection.**

(14) No. 2 rails shall be accepted to at least five (5) per cent. of the whole order. Rails that possess any injurious defects, or which for any other cause are not suitable for the first quality, or No. 1 rails, shall be considered as No. 2 rails; provided, however, that rails which contain any physical defects which impair their strength shall be rejected. The ends of all No. 2 rails shall be painted white in order to distinguish them. Rails rejected under the drop test will not be accepted as No. 2 rails. **No. 2 Rails.**

TESTS ON THE METALLURGY OF STEEL BEING CONDUCTED AT WATERTOWN ARSENAL, MASS.

Tests, inaugurated at the Watertown Arsenal Laboratory under a program approved by the Chief of Ordnance, U. S. A., comprise metallurgical tests on steel ingots and derivative shapes. In carrying out these tests the Laboratory is being aided by a Committee constituted as follows:

Major C. B. Wheeler, Commanding Officer Watertown Arsenal, Watertown, Mass. (ex-officio).

Mr. James E. Howard, Engineer of Tests Watertown Arsenal, Watertown, Mass. (ex-officio).

Mr. James Christie, Consulting Engineer, Wissahickon, Pa.

Dr. Charles B. Dudley, Chemist, The Pennsylvania R. R., Altoona, Pa.

Mr. E. F. Kenney, Metallurgical Engineer, Cambria Steel Company, Johnstown, Pa.

Prof. Edgar Marburg, University of Pennsylvania, Philadelphia, Pa.

Mr. J. P. Snow, Bridge Engineer, Boston and Maine R. R., Boston, Mass.

Mr. A. A. Stevenson, Vice-President, Standard Steel Works Company, Burnham, Pa.

Mr. S. M. Vauclain, Superintendent, Baldwin Locomotive Works, Philadelphia, Pa.

Mr. William R. Webster, Consulting Engineer, Philadelphia, Pa.

Mr. F. W. Wood, President, Maryland Steel Company, Sparrow's Point, Md.

The above Committee has been increased by the addition of

Mr. D. D. Carothers, Chief Engineer, Baltimore and Ohio Railroad, Baltimore, Md., and

Mr. Chas. S. Churchill, Chief Engineer, Norfolk and Western Railway, Roanoke, Va.,

as members since the time of its formation.

Material received for this series of tests includes the metal from a number of heats of acid Bessemer rail steel, acid open-hearth rail steel, and parts of ingots, hammered and rolled tire metal, of acid open hearth product.

Five Bessemer heats of six ingots each made for this research work have furnished material enumerated and described as follows:

	Treatment of Ingots.	Time in furnace, hrs. min.	Remarks.
Heat No. 1.	A—Cooled in vertical position without soaking.		
	B—Soaked and then cooled in vertical position.	1:30	
	C—Charged hot and soaked as usual.	1:30	Rolled into blooms, then cut in center at shears.
	D—Charged hot and soaked as usual.	1:30	Rolled into 75-lb. rails.
	E—Laid on side to cool immediately after stripping, head side up. Reheated.	6:00	Rolled into blooms; then cut in center at shears.
	F—Laid on side to cool immediately after stripping, head side up. Reheated.	6:00	Rolled into 75-lb.; rails.
Heat No. 2.	A—Same as in Heat No. 1.		
	B— " " " " No. 1.	1:30	
	C— " " " " No. 1.	1:30	Same as in Heat No. 1.
	D— " " " " No. 1.	1:30	" " " " No. 1.
	E—Laid on side to cool immediately after stripping, web side up. Reheated.	9:00	" " " " No. 1.
	F—Laid on side to cool immediately after stripping, web side up. Reheated.	9:00	Rolled into 75-lb. rails.

	Treatment of Ingots.	Time in furnace, hrs. min.	Remarks.
Heat No. 3.	A—Same as in Heat No. 1.		
	B— " " " " No. 1.	1:30	
	C— " " " " No. 1.	1:30	Same as in Heat No. 1.
	D— " " " " No. 1.	1:30	" " " " No. 1.
	E—Laid on side to cool immediately after stripping, base side up. Reheated.	9:00	" " " " No. 1.
	F—Laid on side to cool immediately after stripping, base side up. Reheated.	9:00	Rolled into 75-lb. rails.
Heat No. 4.	A—Charged hot and soaked as usual.	1:30	Rolled through third roughing pass.
	B—Charged hot and soaked as usual.	1:40	Rolled through fifth roughing pass.
	C—Charged hot and soaked as usual.	1:50	Rolled through squabbing pass.
	D—Charged hot and soaked as usual.	2:00	Rolled through first forming pass.
	E—Charged hot and soaked as usual.	2:10	Rolled through second forming pass.
	F—Charged hot and soaked as usual.	2:20	Rolled through finishing pass.
Heat No. 5.	A—Charged hot and soaked as usual.	1:30	Rolled three passes in bloom mill.
	B—Charged hot and soaked as usual.	1:30	Rolled six passes in bloom mill.
	C—Charged hot and soaked as usual.	1:30	Rolled nine passes in bloom mill.
	D—Cooled in vertical position. Reheated.	8:00	Rolled three passes in bloom mill.
	E—Cooled in vertical position. Reheated.	8:00	Rolled six passes in bloom mill.
	F—Cooled in vertical position. Reheated.	8:00	Rolled nine passes in bloom mill.

NOTE.—The letters A, B, C, D, E and F represent the ingots of the respective heats; A being the first, B the second, etc.

In the open hearth metal two heats are represented, from one of which there were taken for this experimental work six ingots, from the other three ingots.

The test material is described on the diagrams which next follow:

DIAGRAM SHOWING LOCATION AND TREATMENT OF COBBLES FOR WATERTOWN TESTS.

Series "A."

Ingot No. 1. Heated to 1040° C. Bloomed to $8\frac{3}{4}$ by $10\frac{1}{2}$ ins. Passes 19. Time, 3 min. 30 sec. Bloom finished at 950° C.

Bloom	1st roughing	2nd roughing	3d roughing	4th roughing

Ingot No. 2. Heated to 1050° C. Bloomed to $8\frac{3}{4}$ by $10\frac{1}{2}$ ins. Passes 19. Time, 3 min. Bloom finished at 980° C.

5th roughing	6th roughing	7th roughing	1st intermediate

Ingot No. 3. Heated to 1050° C. Bloomed to $8\frac{3}{4}$ by $10\frac{1}{2}$ ins. Passes 19. Time, 3 min. 40 sec. Bloom finished at 950° C.

2nd intermediate	3d intermediate	Leading	Finishing

Ingot No. 4. Heated to 1050° C. Bloomed to $8\frac{3}{4}$ by $10\frac{1}{2}$ ins. Passes 19. Time, 2 min. Bloom finished at 940° C.

4th roughing	3d roughing	2nd roughing	1st roughing	Bloom

Ingot No. 5. Heated to 1040° C. Bloomed to $8\frac{3}{4}$ by $10\frac{1}{2}$ ins. Passes 19. Time, 2 min. 30 sec. Bloom finished at 980° C.

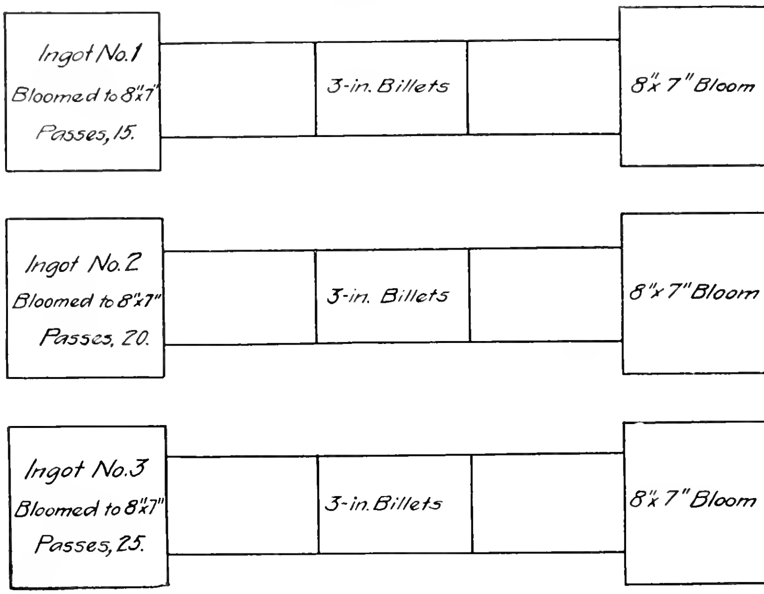
1st intermediate	7th roughing	6th roughing	5th roughing

Ingot No. 6. Heated to 1025° C. Bloomed to $8\frac{3}{4}$ by $10\frac{1}{2}$ ins. Passes 19. Time, 2 min. 35 sec. Bloom finished at 985° C.

Finishing	Leading	3d intermediate	2nd intermediate

DIAGRAM SHOWING LOCATION AND TREATMENT OF BLOOMS AND BILLETS FOR WATERTOWN TESTS.

Series "B."



The material of open hearth cast steel consists of:

1 long ingot split.
1 pipe discard.
1 ingot punched.
1 ingot punched and beaked.
1 rolled tire, new.
1 rolled tire, old.

The tests on the above material will be made for the purpose of ascertaining questions of structural soundness and continuity of the steel, and will embrace a complete series of physical tests, microscopic and chemical examinations.

SOME RESULTS OF THE TESTS OF STEEL RAILS IN PROGRESS AT WATERTOWN ARSENAL.

By James E. Howard.

Tests of steel rails form a part of the program of work of the Testing Laboratory on the subject of steel ingots and derivative shapes, a subject on which the laboratory is working in coöperation with and aided by a Committee on Metallurgical Tests.

The following are members of the Committee:

Major C. B. Wheeler, Commanding Officer, Watertown Arsenal (*ex-officio*).

Mr. James E. Howard, Engineer of Tests, Watertown Arsenal (*ex-officio*).

Mr. William R. Webster, Consulting Engineer.

Mr. Edgar Marburg, Professor of Civil Engineering, University of Pennsylvania.

Mr. James Christie, Consulting Engineer.

Dr. C. B. Dudley, Chemist, Pennsylvania Railroad.

Mr. E. F. Kenney, Metallurgical Engineer, Cambria Steel Company.

Mr. J. P. Snow, Bridge Engineer, Boston and Maine Railroad.

Mr. A. A. Stevenson, Vice-President, Standard Steel Works Company.

Mr. S. M. Vauclain, Superintendent, Baldwin Locomotive Works.

Mr. F. W. Wood, President, Maryland Steel Company.

The laboratory had received valuable material for testing from Mr. R. Trimble, Chief Engineer of Maintenance of Way, Pennsylvania Lines west of Pittsburg, in addition to that contributed by members of the Committee on Metallurgical Tests.

The present tests include those made on rails taken from the track in which certain of the effects due to service conditions were investigated; and also tests and examination of the metal with reference to its state of structural soundness or continuity.

Under the first group, pertaining to rails from the track, tests of transverse strength were made showing the difference in beha-

vior according to the direction of loading. Old rails commonly retain their strength and ability to deflect when the base is on the tension side of the bend. When the metal of the head is put into tension, however, an old rail usually ruptures under diminished load and displays brittleness in the fracture. This brittleness results from the action of the wheel pressures which cause flow of the metal next the running surface of the head and the ductility of the metal having been exhausted in this manner, none remains

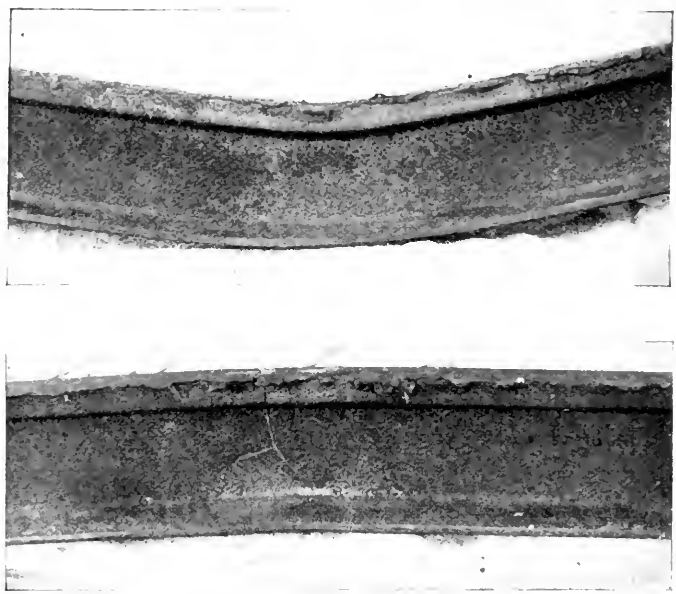


FIG. 1.—Showing difference in bending qualities of rail taken from service according, to the direction of bending. Brittleness displayed when loaded with the head in tension; toughness, when the base is on the tension side of the bend.

to meet the requirements of bending. Although the metal of the head is worn by abrasive action, yet the effect of flow precedes such abrasive loss and impairs the ductility of all grades of steel yet examined.

Annealing the metal restores the ductility, also planing away the affected zone restores the ability of the rail to bend normally.

The difference in bending qualities is shown in Fig. 1; the upper piece of rail bent without fracture while the lower piece failed in a brittle manner. These two pieces from the same rail were bent with the base and with the head in tension respectively. It not infrequently happens that old rails which will bend through an angle of 20° or more without rupture when the base is on the

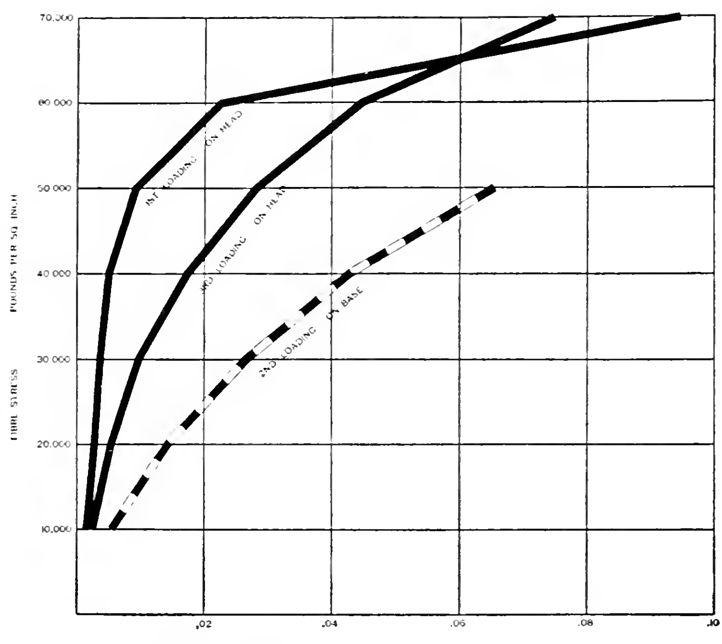


FIG. 2.—Diagram showing the effect of overloading and loading in reversed directions on the development of permanent sets in a 100-lb. steel rail. Span, 54 ins.

tension side, will bend only two or three degrees when the head is brought into tension.

It is well known that when steel is overstrained in one direction the elastic limit in the opposite direction is impaired. Permanent sets will under these conditions appear early when the direction of loading is reversed. This feature is of interest in connection with the subject of cold straightening of rails. Such straightening necessarily impairs the elastic limit of the metal and

permanent sets, although of limited extent, may be expected to develop under comparatively low loads.

Fig. 2 illustrates features of this kind. The curve of permanent sets developed during the first loading of the rail is shown on the diagram, then the sets which formed when the rail was reversed in position and loaded on the base. After this the rail was returned to the first position, now being loaded on the head, and the intermediate line of the diagram resulted. Repeated alternate stresses modify the shapes of the curves.

When steel is strained up to or beyond the elastic limit lines

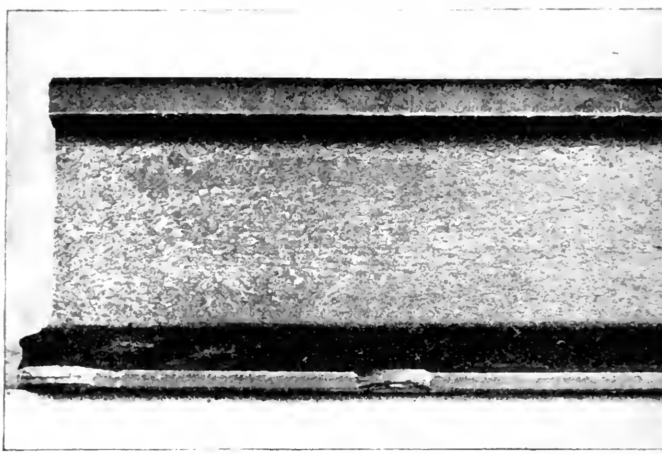


FIG. 3.—Side view of rail after rupture in the testing machine, showing lines of scale disturbed on the web.

of scale start off the surface. Fig. 3 shows the appearance of the web of a rail, on which the scale was disturbed when the sample was fractured in the testing machine. Gagging leaves marks of this kind but less generally distributed. While overstraining inaugurates changes which tend toward the ultimate rupture of the metal, still no case has come to the notice of the writer where the rupture of a rail could be directly attributed to the overstraining once applied in the process of straightening.

Figs. 4, 5, 6 and 7 are views of the same piece of rail. Fig. 4 shows a side view from which may be seen that a crescent shaped piece was detached when this rail was ruptured in the testing

machine, the head being on the tension side. The fracture was a brittle one. Short lines of transverse fractures were formed on the running surface of the head as shown rather indistinctly

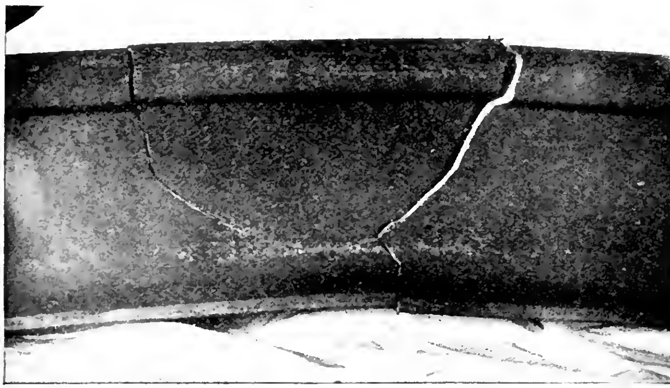


FIG. 4.—Side view of rail fractured in the testing machine; crescent-shaped piece detached. Rail from service.

in Fig. 5, at the edge of the head. The crescent shaped fragment was detached by a fracture having its initial point as shown by the

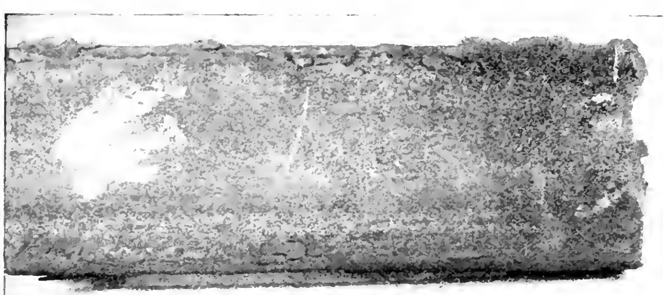


FIG. 5.—Running surface of head, crescent-shaped piece of Fig. 4, showing short transverse cracks developed in metal at edge of head.

arrow in Fig. 6, extending around the curve and terminating at the running surface of the head as shown in Fig. 7.

Another illustration, an end view of a rail broken in the test-

ing machine, Fig. 8, shows upon inspection that the fracture began at the corner of the head at the place indicated by the arrow. It is an advantage to know where a given line of rupture had its origin since this information aids in judging of the cause which contributed toward or located the fracture.

The metal just below the running surface of the head is disturbed by the wheel pressures, and fractured rails commonly show to the eye a difference between the character of the fractured

FIG. 6.—Fractured surface of crescent-shaped piece of Fig. 4, initial side. Fracture began at place indicated by arrow.

FIG. 7.—Fractured surface of crescent-shaped piece of Fig. 4, final side. Fracture ended at running surface of head.

surface in that vicinity and that of the remaining part of the head. An examination of the structure under the microscope still further emphasizes the effect of the wheel pressures.

The normal micro-structure of the metal of the head of a rail, having the same brand mark as that illustrated in Fig. 8, is shown in Fig. 9. In the vicinity of the running surface of the head the appearance of the grain is shown in Fig. 10, this micro-photograph having been taken where the metal had been affected by the tread

of the wheels. Fig. 11 shows the shape of the grain on the side of the head where the rail had been affected by the flanges of the wheels.

Metal in the condition shown by these micro-photographs is comparatively brittle, its toughness having been exhausted by the cold flow caused by the wheel pressures. Annealing restores the grain to its normal shape and restores the ability of the rail to endure the usual amount of bending. If abrasive wear kept apace with this change in structure there is reason for believing an old

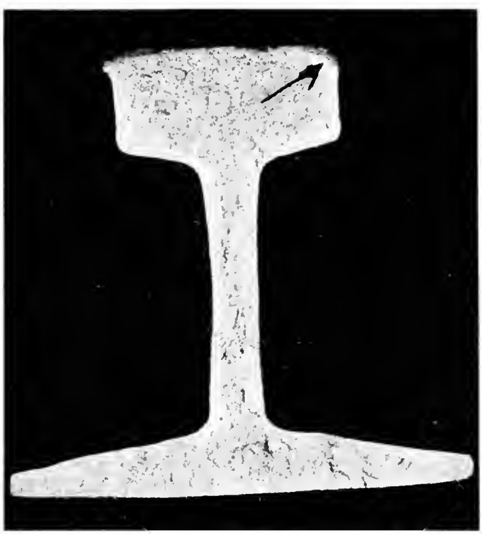


FIG. 8.—Fractured surface of rail broken in the testing machine. Fracture began at corner of head at place indicated by arrow.

rail would retain its ability to bend in either direction with equal success, but such an example has not been met in the tests which are before the writer.

There is another cause which promotes rupture witnessed in the heating of the surface of the rail head when the driving wheels of a locomotive slip, before starting a train. Showers of sparks accompany such an effort, from which may be judged the high temperature acquired by the immediate surface of the head. The

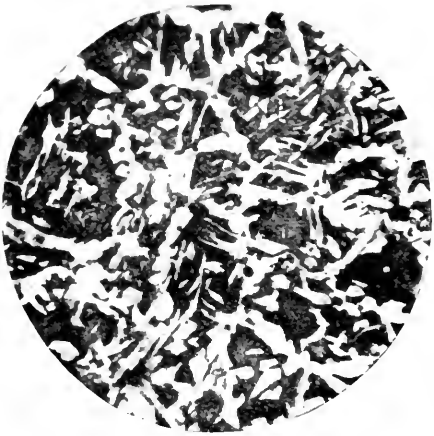


FIG. 9.—Normal micro-structure of head of rail; same brand as rail shown in Fig. 8. Magnification, 94 diameters.

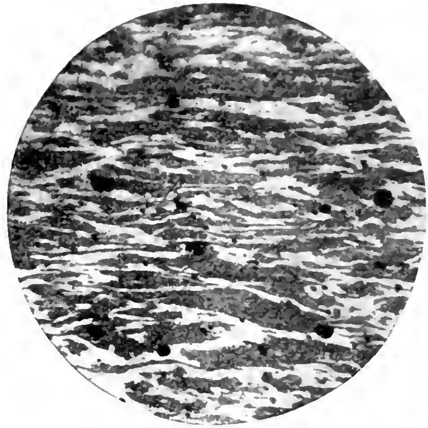


FIG. 10.—Micro-structure of head of rail showing part next running surface affected by wheel pressures. The normal structure of this rail is shown in Fig. 9. Magnification, 94 diameters.

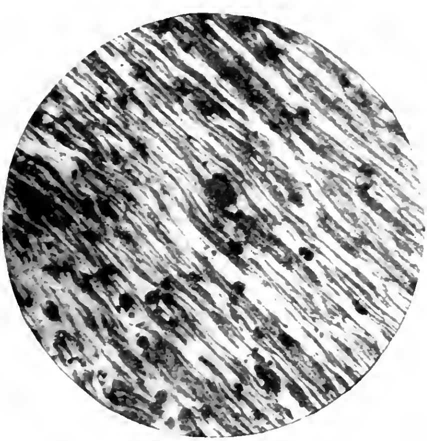


FIG. 11.—Micro-structure of head of rail on side where exposed to flange wear. The normal structure of this rail is shown in Fig. 9. Magnification, 94 diameters.

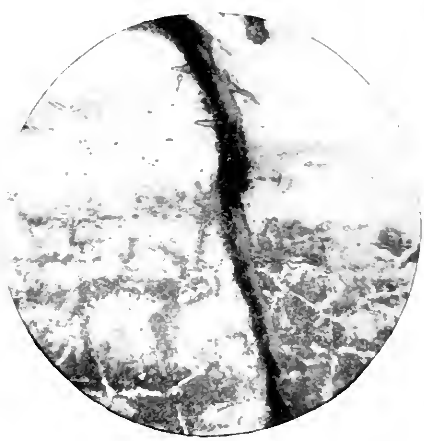


FIG. 12.—Thermal crack in head of rail, caused by slipping of wheels of locomotive. Part of crack next running surface. Longitudinal section of rail. The hardened surface metal is shown by the unetched part of the figure. Magnification, 94 diameters.

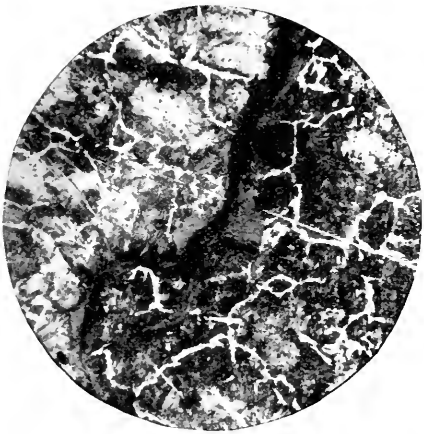


FIG. 13.—Thermal crack in head of rail, caused by slipping of wheels of locomotive. Intermediate part of crack. Longitudinal section of rail. Magnification, 94 diameters.

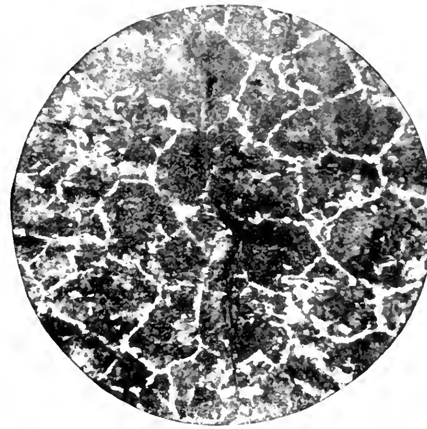


FIG. 14.—Thermal crack in head of rail, caused by slipping of wheels of locomotive. Lower part of crack. Longitudinal section of rail. Magnification, 94 diameters.

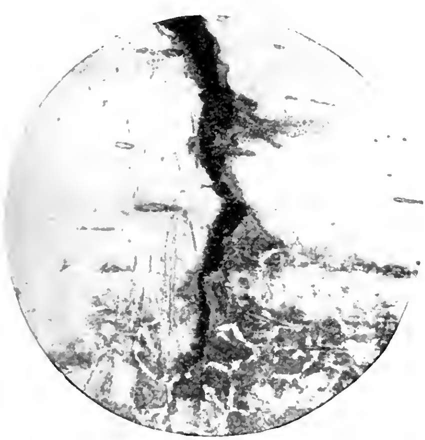


FIG. 15.—Thermal crack in head of rail, caused by slipping of wheels of locomotive. Longitudinal section of rail. Magnification, 94 diameters.

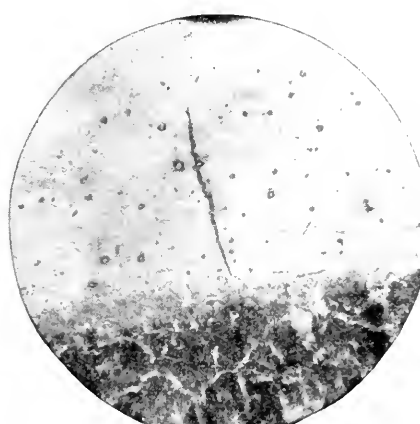


FIG. 16.—Thermal crack in head of rail, caused by slipping of wheels of locomotive. Interior crack in hardened metal next running surface of head. Transverse section of rail. Magnification, 100 diameters.

running surface is much roughened by this action but that result is not the most serious one. The heating of the surface metal is followed by rapid cooling, so sudden, because of the conductivity of the cold metal below, that hardening of the steel occurs. There is also a change from a state of internal compression, existing while the metal is hot, to one of internal tension when the surface metal becomes cold. This results in the formation of cracks in the steel, minute fissures forming in the head, which are a menace to the integrity of the rail as a whole. Such thermal cracks are illustrated

FIG. 17.—Fracture of rail, made in the testing machine. Place of rupture located by an indentation in the steel at edge of flange.

in Figs. 12 to 16 inclusive. Figs. 12, 13 and 14 show different parts of the same crack. The part next the running surface appears in Fig. 12, where the upper part has a curved shape corresponding to the direction of the flow of the surface metal under the wheels. The intermediate part of the crack appears in Fig. 13, while the lower part is shown in Fig. 14.

Another crack in the head of this rail is shown in Fig. 15, the hardened metal being the unetched portion of the cut. Inte-

rior cracks may form as illustrated in Fig. 16, which shows one which had not extended beyond the limits of the hardened zone of metal.

In the harder steels, of which rails of recent manufacture are made, chance injury, such as might occur from a hammer blow indenting the metal, tends to locate the point of rupture when the rail is tested to destruction. Such an indentation of the metal

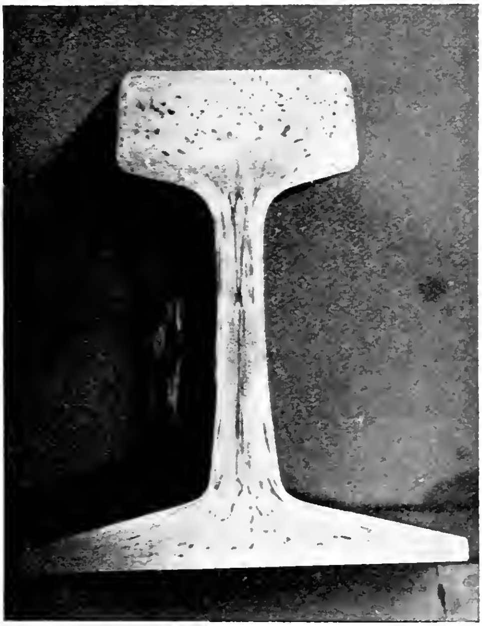


FIG. 18.—Cross-section of 100-lb. steel rail, polished and etched.

occurred to the rail shown in Fig. 17, the edge of one flange of which showed an indented spot. The line of rupture, which formed in the testing machine, began at this spot.

Certain service conditions which tend to cause rupture in steel rails having been referred to, other features will be mentioned which have to do with the metallurgical side of the subject.

It is known that markings usually appear on the cross-section of a steel rail when polished and etched. Figs. 18 and

19 show markings of the kind which are so well known, at least in their appearance. The lines and dots which are found on the end of the rail, as shown in these illustrations, when viewed on a longitudinal surface of the rail appear as streaks, light-colored lines, dark lines, or apparently fine cracks in some cases. The connection between the end markings and the longitudinal streaks is shown in Fig. 20.

From the general disposition of the dots and lines on the end surfaces it may be inferred that the longitudinal streaks would

FIG. 19.—Cross-section of 100-lb. steel rail, polished and etched.

be found at different places when explored at different depths from the head or the base as the case might be. Figs. 21 and 22 show how these streaks are encountered at different depths on a rail the head of which was planed off in several steps. Since the connection between the end markings and the longitudinal streaks appears to be established it would follow, that as end markings are very commonly found, so should longitudinal streaks be commonly found in many rails. Such in fact seems to be the case.

Illustrations of streaks might be multiplied but the general

statement of the case will doubtless be regarded as sufficient. However, two illustrations will be presented showing streaks in the bases of rails. Fig. 23 shows a very streaked appearance found on the base of an English rail of early manufacture, while Fig. 24 shows streaks in the base of a domestic rail. In the latter certain surface furrows appear, with corresponding streaks in the metal immediately below the surface.

Having examined streaks and noted their prevalence in rails,

FIG. 20.—Cross-section of steel rail, polished and etched, showing connection between end markings and longitudinal streaks.

it becomes a matter of deep interest to ascertain whether their presence is of grave importance or not. That they have contributed to many failures in the track there can be no doubt, and their elimination or amelioration is much to be desired if it can be accomplished.

Fins are forced off the edge of a rail and split heads occur and not unlikely such lines of fracture are promoted by the presence of streaked metal. Fig. 25 shows a fracture near the

side of the head which started along the line of a streak. This short crack was not visible until the immediate surface metal had been planed away.

The next photograph, Fig. 26, shows a fracture in the base of

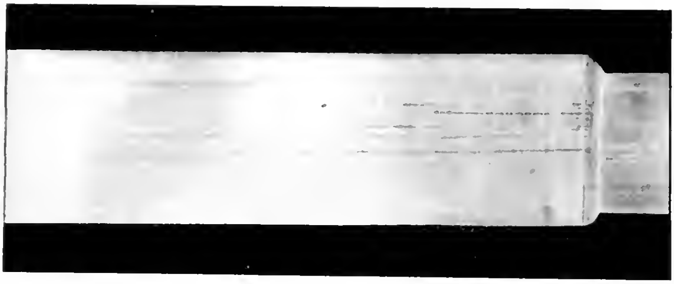


FIG. 21.—Longitudinal streaks on head of rail.

a rail, made in the testing machine, along the line of some streaked metal. It will be remarked that these fractures are transverse ones, that is the metal is strained in a direction at right angles to the tests prescribed for the acceptance of the rails. The large

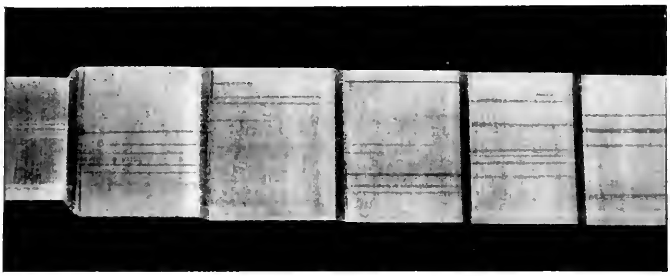


FIG. 22.—Longitudinal streaks in head of rail, at different depths. Same rail as shown in Fig. 21.

number of service fractures which occur in the direction in which these tests have been made might be considered significant and point to the desirability of prescribing tests in the direction in which rails are known to be weak rather than attaching importance chiefly to the direction in which they are known to be strong.

Still another fracture made in the testing machine is shown in Fig. 27, this rupture being made in line of and immediately

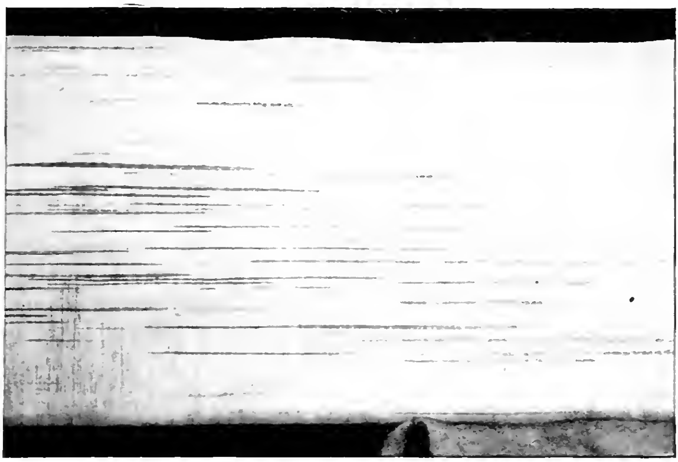


FIG. 23.—Longitudinal streaks in base of steel rail of early English manufacture.

beyond a crescent break which occurred in the rail when in the track. The metal was streaked in this rail and the line of frac-

FIG. 24.—Longitudinal streaks in base of steel rail of domestic manufacture, showing relation of curved surface furrows and streaks immediately below the surface.

ture followed a streak. The initial part of the fractured surface was serrated, as frequently found in streaked metal.

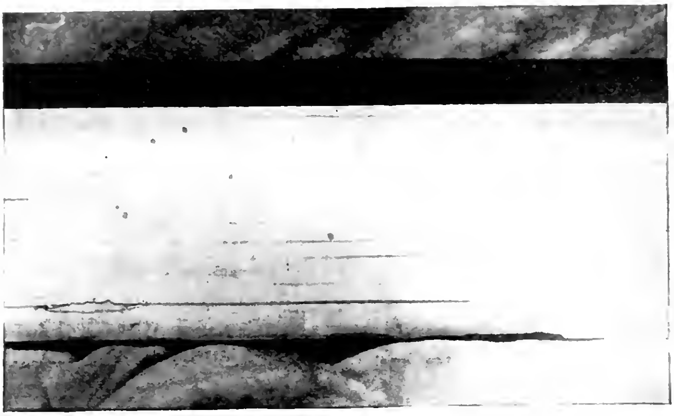


FIG. 25.—Longitudinal streaks in head of rail, showing a fracture started along the line of a streak.

In order to further examine into the question of local weakness or brittleness at the markings witnessed on the cross section

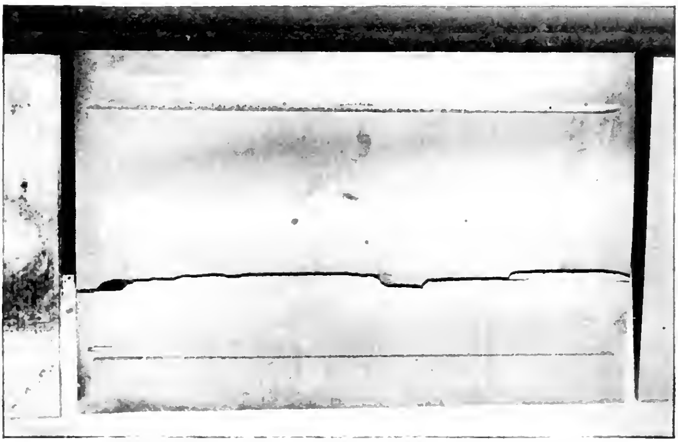


FIG. 26.—Fracture in base of rail, made in the testing machine, along the line of streaked metal.

FIG. 27.—Fracture in the base of a rail, made in the testing machine, at end of a crescent-shaped fracture which occurred in the track.

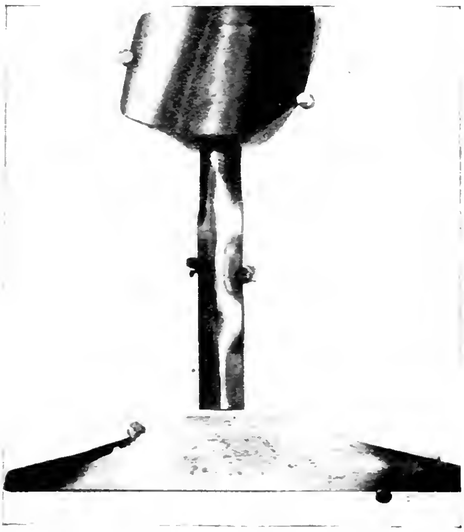


FIG 28.—Thin cross-section of rail, bent locally to show brittleness of metal at the markings, lines, and dots on the etched section.

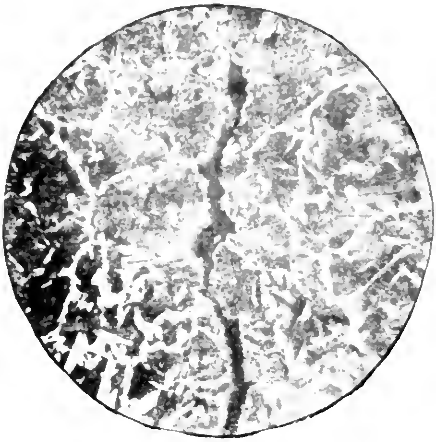


FIG. 29.—Micro-photograph of streak in base of steel rail. Primitive appearance of streak, before straining. Magnification, 120 diameters.

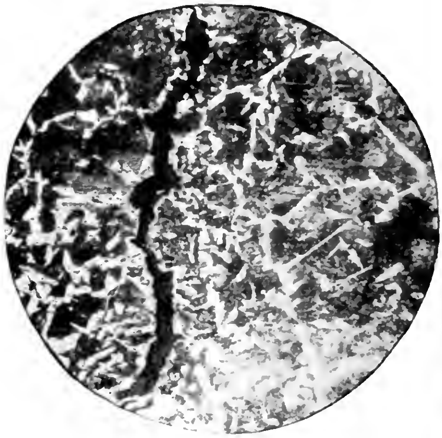


FIG. 30.—Micro-photograph of streak in base of steel rail. Appearance of streak after the metal had been slightly strained, a fissure opening along the streak. Magnification, 120 diameters.

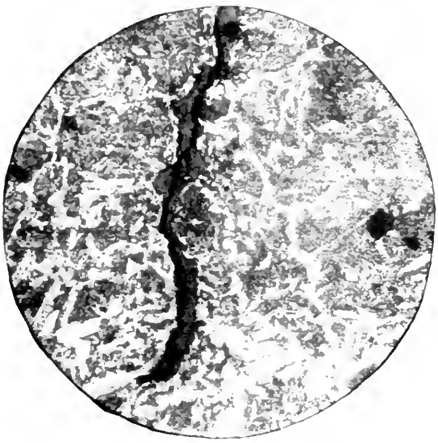


FIG. 31.—Micro-photograph of streak in base of steel rail. Appearance of streak after the metal had been further strained, and the fissure in the streak enlarged. Magnification, 120 diameters.

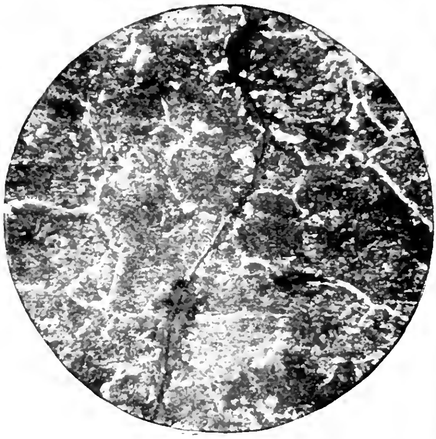


FIG. 32.—Micro-photograph of fissure in another streak in base of same rail as shown by Figs. 29, 30 and 31. Appearance after adjacent part of base had been fractured in the testing machine, along the line of a streak. Magnification, 120 diameters.

of polished and etched rails, the specimen illustrated in Fig. 28 was prepared. A thin section was cut off the end of the rail and bent along the markings which were developed in the usual manner by etching. The metal was brittle at these places, still further establishing the character of the steel as locally deficient in strength and ductility. Following these tests an examination of the base of a rail was made at the line of some streaks the micro-structures of which are shown in Figs. 29 to 32 inclusive.

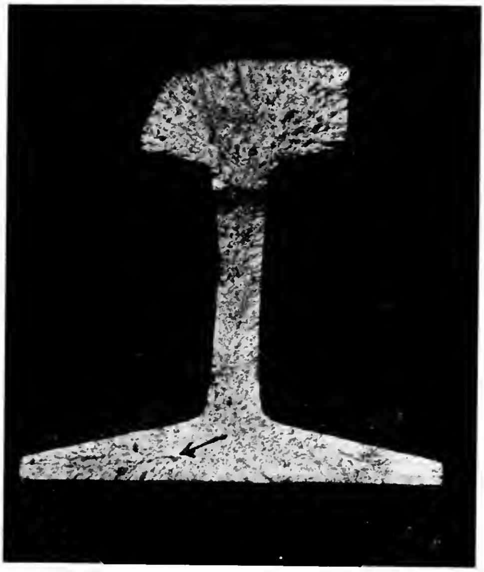


FIG. 33.—End view of rail fractured in the testing machine, showing initial point of rupture in the base.

Fig. 29 is the first of a series of three micro-photographs of a streak, which is shown in this figure in its primitive state. Upon slightly bending the metal, hardly causing a perceptible set, a fissure was opened in this streak as shown in Fig. 30. Upon subjecting the piece of the rail to a greater bending force the fissure was opened wider as it appears in Fig. 31. The extreme brittleness of the metal at this streak is a matter of deep interest. An-

FIG. 34.—End view of rail fractured in the testing machine, showing initial point of rupture in the head, indicated by arrow.

FIG. 35.—End view of head of rail, shown in Fig. 34, after polishing and etching. Initial point of rupture indicated by a well-defined dot developed upon etching.

other streaked line, located near a fracture made during this examination, is shown in Fig. 32, a fissure being developed along this streak.

The fractured surfaces of two other rails are shown in Figs. 33 and 34. Each of these rails began to rupture at an interior point. In the rail shown in Fig. 33, the fracture began near the middle of the depth of the base, at the point indicated by the arrow, while in the rail shown in Fig. 34 it began near the center of the head. The cross section of each rail was examined after polishing and etching, and well-defined dots were found to mark the places where the fractures began. This is shown in Fig. 35, which is a view of the head of the rail shown in Fig. 34 after polishing and etching.

In these results there is further evidence of the structural weakness of the steel at those markings which are developed on the cross section of rails by etching, and which appear as streaks when viewed on longitudinal sections.

The microscopic examination was made in collaboration with Dr. Henry Fay, who recognized the presence of manganese sulphide in certain of the streaks.

[For Discussion of this paper, see page 109.]

A MICROSCOPIC INVESTIGATION OF BROKEN STEEL RAILS: MANGANESE SULPHIDE AS A SOURCE OF DANGER.

By Henry Fay.

During the past few years there has been much talk about the quality of steel rails. On looking over the literature of the subject the writer was not completely satisfied with the reasons advanced for the breakage of rails, and it was decided to make a collection of broken rails from various sources for the purpose of further study. The results herewith reported are only a small part of the work, dealing particularly with those rails in which considerable amounts of manganese sulphide were found. Incidentally, other material than rail steel in which manganese sulphide was found will be mentioned. The results reported have been obtained to a very large extent from personal work conducted at the Watertown Arsenal in collaboration with Mr. J. E. Howard. Mr. Wint, working for the degree of S.B. at the Massachusetts Institute of Technology, under the author's direction, has obtained some important results which will later be published in full and from which some abstracts will be made. The two investigations have been going on simultaneously and the results obtained in each have served to confirm the other in many ways.

The causes variously assigned to rail breakages may be classified under the following heads: (1) improper chemical composition; (2) segregation; (3) improper heat treatment; (4) rolling flaws.

Under the first of these heads the most frequent sources of trouble are: Carbon, which may be too high or too low and about which personal opinion is liable to differ; phosphorus, which will produce brittleness when in excessive amounts; sulphur, which, if present as iron sulphide, will make rolling difficult; and slag, which is an indefinite term embracing oxides or silicates or both. Sulphur when present as manganese sulphide has been declared

to be harmless, but that it is an extremely brittle and perhaps dangerous material will be demonstrated. It must be stated at once that all manganese sulphide is not injurious, in fact most of it is harmless. The conditions under which it becomes harmful will be shown.

So much has been said of segregation, heat treatment, and

FIG. 1.

rolling flaws that they will only be referred to incidentally in discussing breakages in general.

In beginning the work the first piece examined was a small

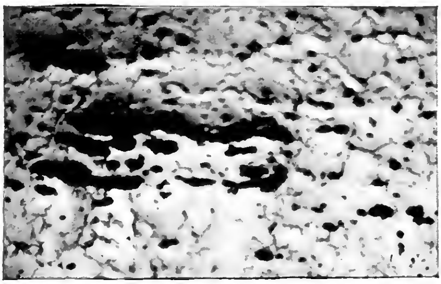


FIG. 2. × 60

section broken out of the foot of a rail and in which there was a good-sized check. The fracture was fine-grained but showed at one point a distinctly fibrous appearance. This is well shown in Fig. 1, in the upper left-hand corner. The metal was cut through just beyond the check and polished to a mirror surface. The surface began to pit badly on a line with the check. Microscopic

examination before etching showed manganese sulphide where the pitting had taken place. Fig. 2 shows the manganese sulphide at the point of the check. Pitting of the surface on polishing which can readily be seen with the eye invariably reveals the presence of manganese sulphide. Slag may also cause pitting but the appearance of the pits is usually quite different. Etching the sample with picric acid revealed a structure which was fine and all that could be desired, showing evidence of good heat treatment. Fig. 3 shows the appearance of the etched specimen. Evidently then, the fibrous part of the fracture was due to the presence of manganese

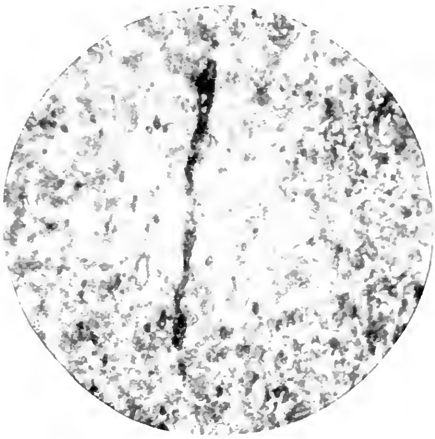


FIG. 3. × 88.

sulphide. This view seemed entirely probable since the check and the sulphide area coincided in position exactly, and further it seemed highly probable that manganese sulphide might be a brittle substance. This latter assumption has since been confirmed and will be discussed in detail later.

The next examinations were made upon a number of rails which had broken in the foot in the crescent form. The samples showed in every case all of the characteristics of the crescent break. Near the top surface of the fractured piece the metal in nearly every case showed a thin layer of apparently more brittle material, and in many cases fine checks, extending in the direction of rolling, similar to the one just described. Sections were cut out of these

crescent breaks, polished, transversely and longitudinally, and examined. In each one manganese sulphide was found in considerable quantities in the polished specimen. It was present in the form of long threads extending in the direction of rolling, as shown in Fig. 4. The etched specimens showed a fine structure, as may be seen in Figs. 5–7. It will be noticed that the manganese sulphide is extended in the direction of rolling and in some cases, as in Figs. 6–8, there are streaks of ferrite running parallel to the manganese sulphide. Whether or not these streaks originally held manganese sulphide in place is not definitely known. It is possible that the sulphide was torn out in polishing. Streaks of this character and also surface streaks deserve further study. There can be no doubt however of the intimate association of the streak and manganese sulphide. The essential point in the case is this: All rails which have broken out in the foot in the form of a crescent contain manganese sulphide alone or manganese sulphide and ferrite drawn out in the direction of rolling. With the structure fine in every case and very little lamellar perlite, the cause for breakage can not be placed on the heat treatment which the rail received. The large amount of manganese sulphide seems to be the cause of these fractures and that this is true will be shown by further evidence which, I believe, will refute the statement made by Snow* that crescent breaks are due to gas seams and rolling flaws. The evidence obtained thus far pointed suspiciously toward manganese sulphide, and this suspicion was confirmed in the examination of a rail which showed a considerable number of streaks. In cutting out the head of the rail in steps, each step showed streaks on machining and these were emphasized by etching. The top of the head of this rail was polished and like the crescent breaks it showed considerable pitting running in the direction of the rolling. On etching with picric acid the streaks appeared bright and were of varying widths running in parallel lines in the direction of rolling. Throughout each streak was found a considerable amount of manganese sulphide associated in some cases with a small amount of silicate and both lying in the ferrite areas. In Fig. 9 is seen one of the narrow streaks with its manganese sulphide embedded in ferrite. A broad streak is

* *Iron Age*, 81, p. 884.

FIG. 4. × 60.

FIG. 5. × 60.

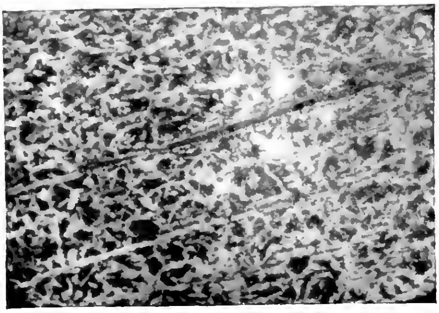


FIG. 6. × 60.

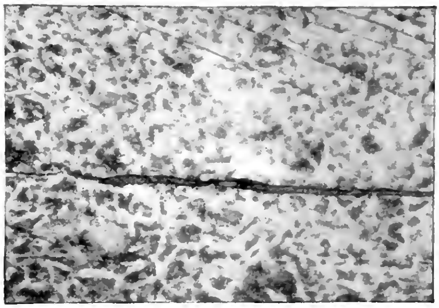


FIG. 7. × 60

FIG. 8. × 60.

seen in Fig. 10. That the light appearance of the streak is due to ferrite is shown by an examination of Figs. 10 and 11. The latter photograph was taken at a spot immediately adjacent to Fig. 10. On etching the ferrite areas remain bright while the perlite is darkened. The distribution of perlite invariably takes place in this way. This distribution, together with a wide and a narrow streak with the embedded sulphide is well shown in Fig. 12. That rails, in which streaks of this character were found, were broken along these lines was known, but the mechanism of the break was not known. Having found these streaks with the associated manganese sulphide on both the top of the head and the bottom of the foot of the rail, it was considered desirable to know whether or not they penetrated through the rail from top to bottom. Transverse sections of the head and foot were then polished and etched. This showed that the penetration of the streak took place only to a limited extent at the top and bottom. Considerable manganese sulphide was found throughout the cross section but the ferrite streak was apparently a surface phenomenon, extending in only a very short distance. An examination of the transverse section of the foot at a point where the streak penetrated into the foot showed manganese sulphide extending in from the surface toward the web. A photograph taken at one of these spots is shown in Fig. 13.

Having conceived a suspicion of the brittleness of manganese sulphide it was decided to test this by subjecting the rail to strain such as it might get in service. The foot was blocked on either side and pressure applied vertically downward.

Examination of the middle edge of the foot at the point of greatest strain was made from time to time to see if any cracks had started. Fig. 14 shows the beginning of a crack which extends alongside of and through the manganese sulphide. It will thus be seen that the brittleness of the material itself has been demonstrated and also that it is cemented only weakly to the steel in which it is embedded. On applying further pressure this crack widened as shown in Fig. 15 and developed in similar areas as shown in Fig. 16.

The whole edge was examined to see whether cracks had started at any other point but absolutely none were found. The specimen was further subjected to strain and it finally broke

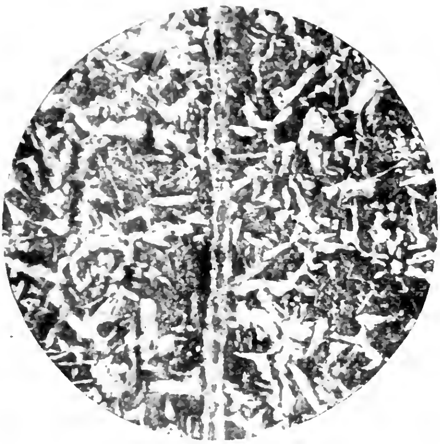


FIG. 9. × 87.

FIG. 10. × 100.

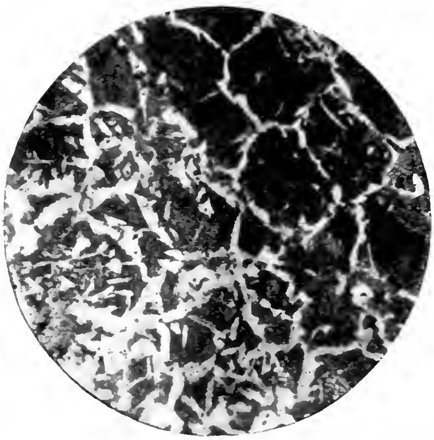


FIG. 11. × 100.

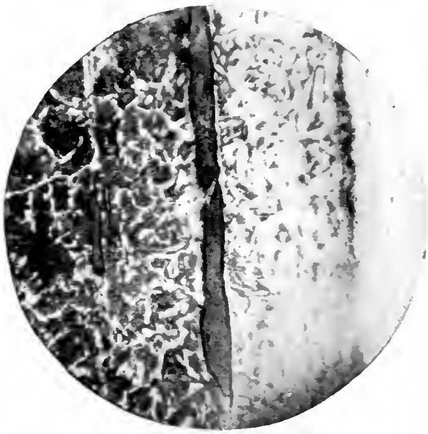


FIG. 12. × 110.

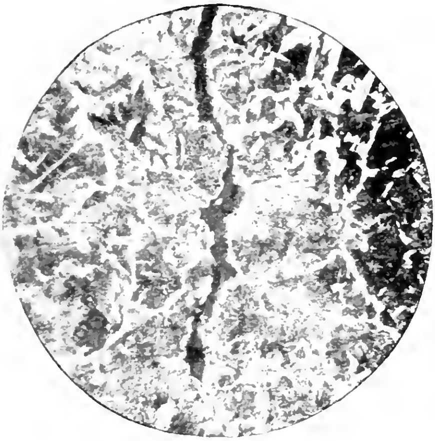


Fig. 13. × 88.

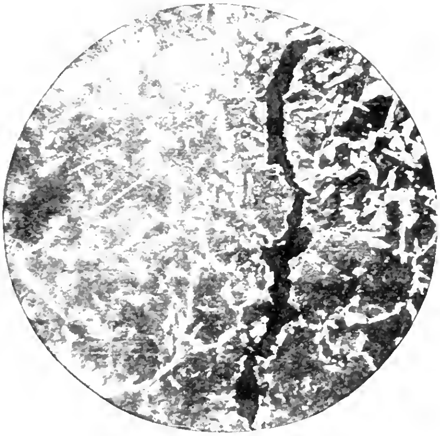


Fig. 14. × 88.

Fig. 15. × 88.

Fig. 16. × 82.

Fig. 17. × 105.

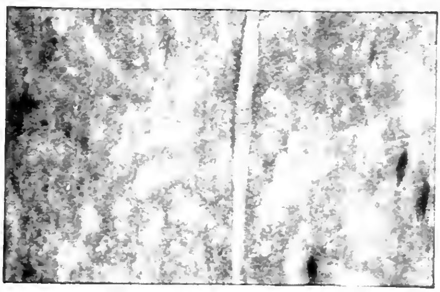


Fig. 18. × 105.

before it had taken any appreciable permanent set. The fracture did not take place at either of the points of which photographs are shown but through a streak adjacent to these two. The metal of the fracture showed a check on the lower side of the foot similar to the one shown in Fig. 1, and extending throughout the fracture. The fractured surface was polished and the long check was found full of manganese sulphide. This observation then not only confirms the suspicion of brittleness of manganese sulphide, but also confirms the relationship between checks and manganese sulphide.

The next logical step in the investigation was to extend this method of attack to other material. Accordingly sections were cut out of some crescent breaks, polished, etched and examined

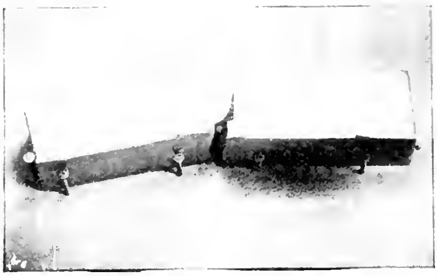


FIG. 19.

for manganese sulphide. Instead of straining so that the crack might be developed along the length of a sulphide area the force was applied in such a way that the strain would be across the direction of rolling of the rail. In this way numerous cracks were developed across the manganese sulphide areas as is shown in Figs. 17 and 18. The fracture of the metal took place after it had taken a slight permanent set. Extreme brittleness was shown here, and greater brittleness possibly would have been shown if the force had been applied so that the cracks could have been developed along the direction of rolling. Fig. 19 shows the fracture of one of the pieces cut out of a crescent break. Its brittle nature is clearly indicated.

Further evidence was procured from a piece of metal cut out of a 12-in. rifle which had broken in service. This metal had shown streaks on machining and an examination showed a large amount

of manganese sulphide in the streaks. The structure was distinctly sorbitic and indicated excellent heat treatment, but, unfortunately, the metal was filled with manganese sulphide. The structure and the sulphide are shown in Fig. 20. An examination of the various sulphide areas was made before and after straining. It was found that cracks developed not only through but around the manganese sulphide. In Figs. 21 and 22 are shown the same area before and after straining with the crack developed and running into a large island of sulphide. In the same specimen the cracks followed the outlines of some of the islands. In Fig. 23

FIG. 20. × 84.

is shown a piece of manganese sulphide which had been cracked around its outline where it had been cemented to the metal, and which has been pushed by means of a needle at right angles to its original position. The significance of the beginnings of cracks in metal of this character is evident now, but it will be made more evident later.

With the evidence thus developed I was prepared to find some connection between many fractures beside the crescent breaks and the presence of manganese sulphide and in this I have not been disappointed. In the beginning of this paper, rolling flaws were spoken of as one of the chief causes of fractures. Only two fractures of this kind have been examined and from the evidence

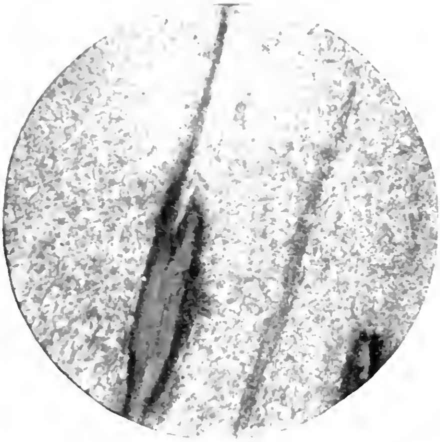


FIG. 21. × 100.

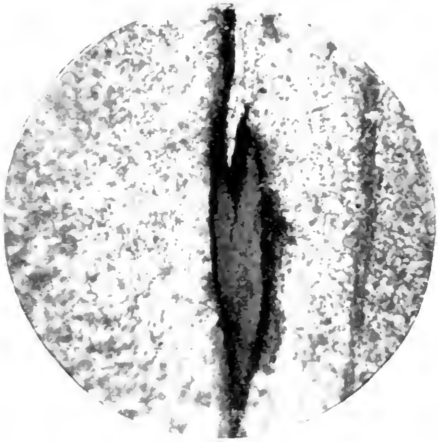


FIG. 22. × 100.


FIG. 23. × 57.

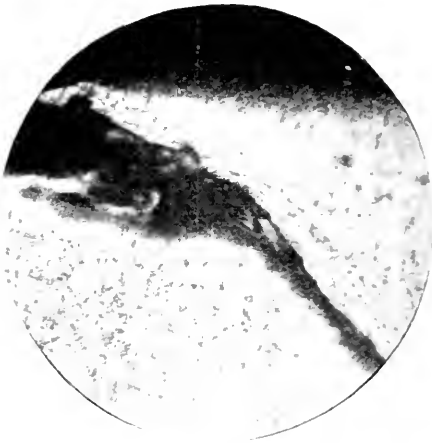


FIG. 24. × 118.

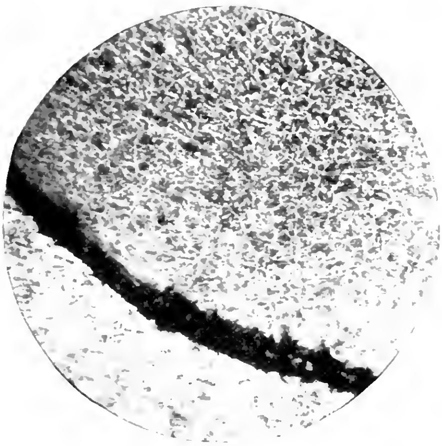


FIG. 25. × 118.

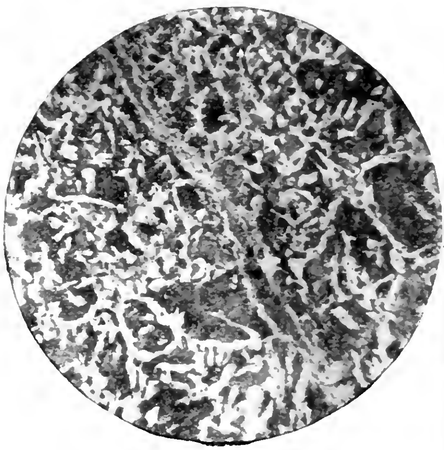


FIG. 26. × 118.

presented by these cases it would be unfair to draw any definite conclusions, but it is significant that the crack developed in each case followed a streak in which manganese sulphide was embedded. In one case the metal examined was cut out of the foot of a rail which had broken in service. The top of the foot was crimped in the process of rolling and the fracture took place at this point. A cross section at the crimped portion showed a crack fairly wide at the surface and narrowing as it proceeded inward. There was considerable flow of metal at the beginning of the crack, but diminishing as it proceeded. Following the crack inward manga-

FIG. 27. × 118.

nese sulphide was found on the walls of the narrowest end and extending beyond the point where the crack stopped was a streak in which manganese sulphide was embedded. This is well shown in Figs. 24–26, which show the beginning and middle portion of the crack, and the streak along which it would have followed. That this crack began in a manganese sulphide area seems highly probable. In the same specimens where the metal was crimped in rolling there was found a large mass of sulphide which extended in a short distance from the surface of the rail, and then extended further in two narrow streaks. With such brittle material it can easily be conceived as the starting point for a crack, and the crack once formed would extend along the two narrow branches. This area is shown in Fig. 27.

In another rail there was a minute crack following a streak in the extreme edge of the foot. Microscopic examination showed manganese sulphide on the walls of the crack and disseminated throughout the ferrite which lay only on one side of it. On the other side were the normal ferrite and perlite areas. Comparison of this photograph, Fig. 28, with Fig. 12 will show a decided similarity except for the crack. In each case manganese sulphide is found with an excess of ferrite on one side and the normal structure on the other.

When manganese sulphide is found in combination with a

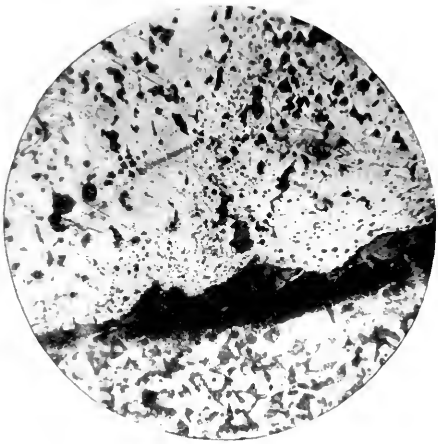


FIG. 28. × 118.

hardened surface the result is decidedly bad, for then cracks are likely to develop not only through the sulphide but also through the hard metal on the surface. Such a combination has come under the author's attention. The walls of the cracks were lined with manganese sulphide and the cracks extended from the hard surface down into the softer metal of the head. Etching with picric acid had little or no effect upon the hard surface, while Kourbatoff's reagent darkened it in a very few minutes. In this piece of metal the hardened surface was filled with cracks and along each crack manganese sulphide was found on the walls. In speaking of Figs. 21, 22, and 23, it was stated then that the significance of the beginnings of cracks in manganese sulphide was

evident, but that it would be more evident later. It will be well to point out the fact here that the rifled surfaces of our large guns are hardened by the high temperature produced by firing. In that case if manganese sulphide be present in considerable amounts it is not at all surprising that the gun has a very short life, for with such brittle material as hard steel and manganese sulphide cracks will inevitably begin and extend until fracture results. The conditions in a rifle then if the sulphur be high are almost identical to this particular case.

Another interesting application of the same idea is to be found

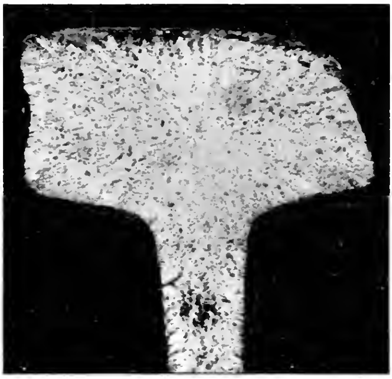


Fig. 29

in a rail head which showed a distinct center of radiation of the fracture as shown in Fig. 29. Mr. Howard called my attention to this and suggested that there might be some connection between the beginning of the fracture and some brittle material. On polishing a cross section of the head distinct pitting took place at a point corresponding to the center of radiation. This pitted spot was found to be filled with manganese sulphide, and the unetched surface is shown in Fig. 30. Etching with picric acid showed segregation and also marked the same spot as is shown in Fig. 31. In two cases only has the center of radiation of a fracture been connected with the presence of manganese sulphide but it will undoubtedly be found in other cases, and the study of this phenomenon will lead to interesting results.

As there has been expressed to me at times some doubt as to

the identity of manganese sulphide it seems to be well to review briefly the literature on the subject before discussing the effect upon the steel industry of the foregoing results.

By purely chemical means Osmond and Werth* in 1885 discovered that sulphur in steels which contain manganese exists in the form of manganese sulphide. In 1888 Osmond† described experiments which showed that steels which are partially dissolved in hydrochloric acid have less sulphur and manganese in the part undissolved than the part which has gone into solution. Stead,‡ independently, confirmed this same observation. As a micro-

FIG. 30. × 57.

scopic constituent it was identified by Professor Arnold as a dove-gray substance, and this was confirmed by Andrews in his various studies of steel rails. Le Chatelier and Ziegler,§ using the Goldschmidt process, prepared and studied steels containing manganese and sulphur and confirmed the statements of Arnold, Arnold and Waterhouse‖, and Stead; Law¶ has subsequently confirmed this.

To make certain that manganese sulphide appears as described by Arnold and others, the author had some pure manganese

* "Theorie Cellulaire des proprietes de l'Acier. *Ann. des Mines*, 1885, [8]. 8, p. 5–85.

† *Ann. des Mines* (8), 1888.

‡ *The Iron and Steel Mag.*, 1905. Vol. 9.

§ *Bull. Societe d'Encouragement*, September, 1902.

‖ *Journal Iron and Steel Inst.*, 1003, p. 136.

¶ *Journal Iron and Steel Inst.*, 1907, 94.

sulphide prepared by precipitation using the method of Olsen* for its preparation. This was melted in an electrically heated vacuum furnace with some pure iron. The two substances were mixed so as to have about 0.08 per cent. sulphur in the resulting 100-gram button. This button was cut through from top to bottom, polished, and examined and it showed the characteristic dove-gray spots of manganese sulphide. It is proposed to roll this material at various temperatures to see if the sulphide can be elongated. The indentity of the dove-gray particles can no longer be doubted, since nothing but pure iron and manganese sulphide were used in the experiment.

FIG. 31.

That iron sulphide is harmful there can be no doubt, but that manganese sulphide is harmless there is much doubt. Le Chatelier and Ziegler state:

> The manganese will absorb all the sulphur, and in this way remove the bulk of it from the metal. It is indeed what has been observed in practice. The other part of the sulphide which remains imprisoned in the metal during its solidification is disseminated in the condition of small grains whose influence cannot be more harmful than that of small bubbles similarly distributed.

Arnold and Waterhouse in a micrographic study of some of Brinell and Wahlberg's steels have shown the harmful influence of iron sluphide and further have shown that some steels with

* *Journal Amer. Chem. Soc.*, 26, 1625.

0.55 per cent. sulphur in the form of manganese sulphide have forged perfectly. Stead casts suspicion on both manganese silicate and sulphide and suggests that their influence be further studied.

The form in which manganese sulphide exists determines whether or not it is harmful. Arnold points out that the appearance depends upon which way the section of metal be cut, i. e., whether it be cut transversely to the direction of rolling, parallel horizontally, or parallel vertically. By an examination of this kind we can get an idea of the shape and dimensions of the sulphide areas. In the crescent breaks which are cut parallel to the direction of rolling there is found usually an elongated ellipsoid

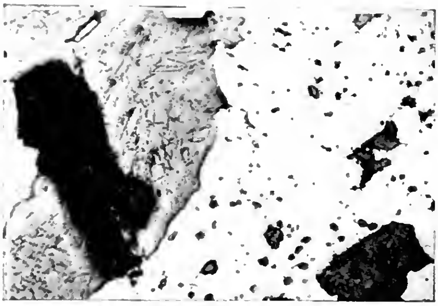


FIG. 32. × 82.

such as that shown in Figs. 4 or 5. The transverse section of this same material will show round dots as in Fig. 2. Such elongated masses are, I believe, distinctly harmful.

Similar forms are found in the steels which show streaks although here there is a tendency toward a shortening and a flattening of the ellipsoid as in Figs. 12, 20 and 21.

In other cases the sulphide may be caught near the surface as shown in the figures showing rolling flaws or in large masses, such as shown in Fig. 32, which is a so-called "wrought iron" rivet which was one of a lot which gave bad results in service.

In one case the author has seen a piece cut out of an axle broken in service about one-half of an inch square. All of these forms are believed to be exceedingly harmful.

On the other hand if the section cut in three directions shows

an appearance such as shown in Fig. 33, there can be no appreciable danger from it, except that the ductility of the metal is lower than it ought to be.

The form in which the sulphide is finally found depends very largely upon the treatment which the metal has received. If the metal has merely been forged it will then appear in small spherical masses. If, on the other hand, rolling has been begun at a high temperature the sulphide areas will be elongated as in the crescent breaks.

In his work on sulphide of iron, Le Chatelier makes the statement that inasmuch as sulphide of manganese often appears crystallized, it has a higher freezing point than any of the other constituents present. This statement has been granted by Arnold and Waterhouse* but has been questioned by Law.

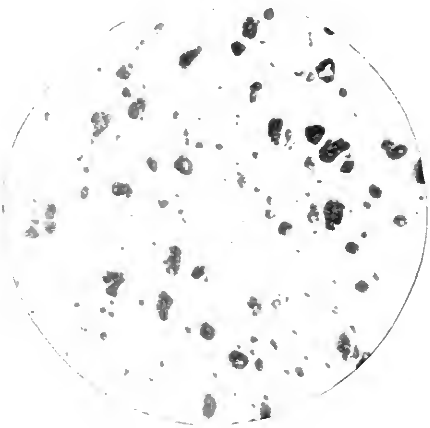


Fig. 33. × 70.

It seemed highly desirable to determine the melting point of manganese sulphide in order to establish the conditions under which it might be elongated as found in the crescent breaks. For this purpose pure, green manganese sulphide was prepared and dried in such a way as to prevent oxidation. It was then melted in an electrically heated furnace while a current of dry hydrogen sulphide was passed in to prevent oxidation. A thermo-couple

* *The Metallographist*, 6, 310.

consisting of a platinum, platinum iridium junction was inserted into the liquid mass and readings of the galvanometer were taken at intervals during the cooling. The freezing point was fairly sharp at 1162° C. This was checked by a melting point on the same sample. Below the melting point the mass was decidedly plastic through a considerable temperature interval. This melting temperature will be influenced to some degree by the amount of manganese in the steel. Manganese sulphide is probably capable of dissolving a certain amount of manganese and this would lower the freezing point to some extent. It would require a study of the equilibrium between manganese and manganese sulphide to definitely decide this. Inasmuch as the color of the sulphide varies with the amount of manganese present, the above assumption seems to be warranted. The specific gravity of this sample was 3.966.

This experiment definitely refutes Le Chatelier's statement quoted above, and further explains the elongated masses of the sulphide. Rail steel solidifies at about 1450° C. If rolling begins at any temperature above 1162° the manganese sulphide will still be liquid from the temperature at which rolling began to 1162° C., and below this temperature being in a plastic condition it is elongated in the direction of rolling. Where rolling pressure is exerted on three sides it would appear in elongated threads. If, however, rolling pressure is exerted on only two sides it might be expected to be found in flattened elongated masses. This latter conclusion has not been verified, but examination of sections of flat bars will soon be made. Steels which are high in sulphur should not for this reason be rolled at too high a temperature for if much manganese sulphide has been entrapped it will surely be rolled out into a form which will ultimately lead to trouble.

That manganese sulphide when existing in certain forms is a harmful constituent of steels can no longer be doubted. The remedy seems to be very simple and on this account there should be no anxiety.

Specifications should be so drawn as to limit the amount of sulphur in the steel. At the present time most of the specifications do not even mention sulphur. Having done this, the next step is to allow the metal to stand a longer time after the addition of the ferro-manganese. With the specific gravity of manganese sulphide

3.966 and steel 6.82 it should rise to the surface and be skimmed off with the slag if given sufficient time. Usually this time interval between charging of the ferro-manganese and the pouring of the ingot is very short. The desire of the manufacturer to increase his output has led him to cut down this interval to the shortest possible limit with the natural consequence of a large number of broken rails. A longer time interval will allow the metal to purify itself.

If, on the other hand, it is not permissible to start with a low sulphur ore, or to allow a sufficient time interval for the removal of the manganese sulphide, resort must be had to electric refining of the molten metal by means of a basic slag.

In conclusion I wish to express my indebtedness to Dr. P. H. Dudley, Dr. C. B. Dudley and Mr. J. W. Snow for samples of broken rails; to Mr. Wint for devoted service and to Mr. G. R. Norton for his skilful photographic help.

[For Discussion of this paper, see page 109.]

RAIL FAILURES—MASHED AND SPLIT HEADS.

By M. H. Wickhorst.

During the last year or two considerable attention has been given to steel rails, their service performance, failures, design, specifications, etc. This paper is given as a contribution to the general subject and deals with one kind of failure known as "mashed" or "split" head, generally known to trackmen as "piped" rail, although it is in most cases not a piped rail properly so called. I desire to discuss this type of failure in relation to segregation.

Fig. 1.—100-lb. Rail with Split Head.

Fig. 1 shows an etched section of a 100-lb. rail which failed, due to the mashing and splitting of the head. This kind of failure first shows itself to the trackmen by the development of a dark streak along the head of the rail from several feet to several yards long. At the same time the head starts to sag down on one side or perhaps both sides, spreading somewhat. The section illustrated in Fig. 1 was from a 100-lb. rail laid about October 15, 1907, taken up December 27, 1907, being in service approximately 70 days. The marking indicated it to be the top rail of the ingot. It was laid as the outside rail of a three degree curve and was the seventh rail from the beginning of the curve. The rail was

removed on account of a black streak showing on the head of the rail and in addition to this black streak there could be detected by the eye a sunken portion on the outside part of the head about two or three feet from one end and extending over a length of six or seven feet.

Chemical tests were made from two different samples of this rail, one being taken from where the head seemed to be in the worst condition, which was 5 ft. 6 ins. from the leaving end; the other being taken off the other end of the rail where it was apparently in good condition. These sections we will call "A" and "B" respectively. From each section borings were taken parallel

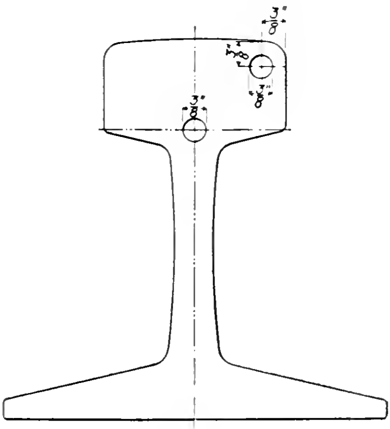


FIG. 2.

with the length of the rail, one sample in each case being taken from an upper corner of the head and the other sample from near the junction of the head and the web. The borings were taken about as shown in Fig. 2. The results of analyses are shown in Table I.

TABLE I.—ANALYSES OF STEEL FROM 100-LB. RAIL, PER CENT.

	Carbon.	Phosphorus.	Sulphur.	Manganese.
A-Head	.51	.082	.060	1.24
A-Web	.81	.117	.152	1.38
B-Head	.56	.110	.076	1.41
B-Web	.64	.163	.114	1.55

It will be noted that there was very considerable segregation of the carbon, phosphorus and sulphur in both cases. In addition to the chemical analysis we made some tensile tests of a piece taken close to sample "B," which are of interest. Two of the pieces were transverse pieces taken from the middle of the head and from immediately below and called samples "1" and "2", respectively. The third test piece called No "3" was a longitudinal test piece taken from the middle of the head and was the usual test piece ½ in. in diameter by 2 ins. gauge length. Results of these tests are given in Table II.

TABLE II.—TENSILE TESTS OF STEEL FROM RAILS.

TRANSVERSE TEST PIECES.

	Dimensions.	Tensile Strength. Lbs. per sq. in.	Elongation.
No. 1	.696 x .292 in.	50,095	None.
No. 2	.507 x .293 in.	58,600	None.

LONGITUDINAL TEST PIECE.

	Elastic Limit. Lbs. per sq. in.	Tensile Strength. Lbs. per sq. in.	Elongation in 2 inches.	Reduction of area.
No. 3. .505 in. Diameter	77,200	108,250	5.3 per ct.	7 per ct.

It will be noted that while the longitudinal test piece shows a good tensile strength, the elongation and reduction in area are very low. The transverse tests show low tensile strength and no elongation.

From the above examination, the splitting of the head is probably to be explained as follows: The load comes on the head of the rail more or less to one side due to the wear of the tread of the wheel or canting of the rail. This introduces transverse tensile stresses in the head of the rail, which are greatest at the top. On account of the bad segregation and the consequent weak condition of the metal, the material is unable to withstand the internal tensile stress and a crack develops internally and gradually increases until finally it breaks through on the under side of

the head where the latter joins the web. The top surface of the head for a depth of about ¼ in., being of normal composition and having the effect of the rolling, is in good physical condition, and it flows instead of developing a crack. This explains why the crack starts internally about ¼ in. below the top.

We have also examined some 75-lb. rails which failed after six or seven years' service by the heads splitting as shown in Fig. 3. Analyses of borings from these three rails taken as explained above gave the results shown in Table III.

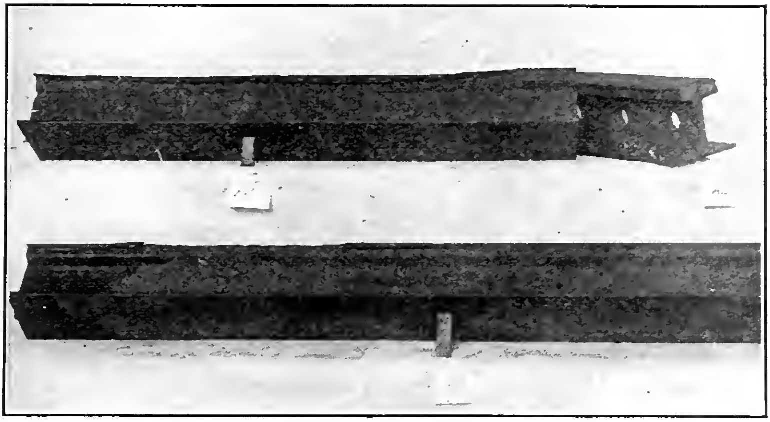


FIG. 3.

TABLE III.—ANALYSES OF STEEL FROM 75-LB. RAILS, PER CENT.

	Carbon.	Phosphorus.	Sulphur.	Manganese.
75-lb. A2-Head	.47	.078	.032	1.08
75-lb. A2-Web	.75	.184	.074	1.13
75-lb. B2-Head	.58	.097	.042	1.38
75-lb. B2-Web	.66	.146	.078	1.24
75-lb. No. 1-Head	.48	.085	.028	1.27
75-lb. No. 1-Web	.66	.175	.060	1.49

It will be noted that here again there is considerable segregation of carbon, phosphorus and sulphur.

Segregation occurs mostly in the top rail of the ingots, where

there is also more or less sponginess of structure, and split heads, that is, cracks which develop after the rails are put into service, are probably due to this condition of segregation and sponginess. Pipes properly so called exist in the rail as rolled, but probably the great majority of failures called pipes by trackmen are really splits occurring as above described.

[For Discussion of this paper, see page 109.]

SOME NOTES ON THE RAIL SITUATION.

By E. F. Kenney.

The extended investigation and wide discussion of the steel rail question during the last year has resulted in a much clearer conception of the problem than has been possible heretofore. The exchange of ideas between the makers and users has had a broadening effect on both, and has resulted in a disposition on the part of both to see a little of the other side of the argument. The users are beginning to admit that the trouble is not all in the rail, that all of the other elements of the tracks, subgrade, ballast, ties, etc., enter into the problem, and the makers are quite willing to admit that there are limitations in the art of steel making as known to-day.

Up to about ten years ago there was practically no rail problem. The breakage of rails was comparatively slight and, except in sharp curves, the wear was such that no one worried much about the behavior of the rails. The factor of safety was large enough to take care of the unusual conditions. But with the coming of the extremely heavy wheel loads and the greater tonnage and speed which have been adopted in the last few years, the stresses and shock in the rail have increased. Moreover, the difficulty in keeping up good track has increased and since the condition of the roadbed as to line, surface, etc., has an immediate effect in the stresses and shock in the rail, these elements, combined with the direct effect on the rail from the increased loads and speed, have so increased the rail's work that the factor of safety is about used up and the great number of failures to-day shows that we are on the ragged edge. While the weight of the rails has been increased from time to time the increase has not, particularly in the last fifteen years, been in proportion to the increase of the loads, tonnage and speed, and in the support afforded the rail there has been absolutely no betterment. The ties, ballast and sub-grade are the same to-day as they were when the service conditions were much less severe.

Section.

In almost any engineering structure, an increase in the load and stresses is met by the engineer by an increase in the resistance, by means of greater sections and stronger members; but there seems to be very strong opposition to an increase in the amount of metal in the rail. There are various reasons given for this, but none of them are convincing. One commonly advanced is the fact that rails of lighter weight, for instance 70 lbs. per yd., show less breakage than the heavier rails. While there has been little evidence produced on this point, it is probably true. The light flexible rail will relieve itself by bending until it gets support, whereas the heavier rail, being stiffer, will stand up and take the stresses. This difference is shown in the behavior of the two sections under the drop test. Under the same blow the 100-lb. rail will break very much oftener than the 70-lb. rail, but the deflection caused by the blow will be much less. In track we can keep better line and surface with the 100-lb. rail than with the lighter rail but in doing this we put more work on the rail and less on the other elements of the track. As the average cost of rail on American railroads is only about 7 per cent. of the total maintenance charges, and as it is well known that the cost of maintaining track laid with 100-lb. rails is decidedly less than that laid with 85- or 70-lb., it seems quite probable that the adoption of much heavier rail sections than those used to-day would be distinctly an economy because of the reduction of the cost of maintaining the track. Aside from economy it would unquestionably be an advance on the side of safety. The desirability of stiff track is so well known and understood that some of the engineers working on the problem have shown a disposition to get the extreme value in stiffness that is possible with the amount of metal they are willing to use. This unquestionably makes better riding track and reduces cost of maintenance, but they seem to forget in this hunt for stiffness that in disposing their metal so as to get the maximum moment of inertia, they are crippling the section in its details. Now over ninety per cent. of the rails which fail do not fail as girders, but in their details; the split heads and broken bases are in no case the result of girder action. Regarding the split head, which is the type of failure probably causing more trouble than all the others combined, the following figures may be instructive. The figures are

official, having been furnished by the railroads, and show the number of rails removed from track as having failed out of each 1,000 tons rolled. All of the roads are lines having heavy traffic, Road A having the heaviest tonnage in the country.

Road A uses a deep-headed rail while B and C use a shallow-headed rail. The number of 100-lb. rails removed from track in one year per 1,000 tons rolled is 0.66 for Road A, and 12.59 for Road C. The number of 85-lb. rails removed from track in one year per 1,000 tons rolled is 0.59 for Road A, 6.02 for Road B and 8.08 for Road C. The percentage of failures was ten to twenty times as great for Roads B and C as for Road A, and of these failures on the last named roads ninety per cent. were in the head, i. e., splitting, often spoken of as piping, while this type of failure was almost unknown on Road A with the deep-headed rail. The shallow head is an element of weakness which must be avoided if we are to escape this type of failure, and the necessary stiffness should be provided not by spreading the metal out so thin as to weaken the head and base of the rail, but by the addition of enough metal to furnish the required girder strength without weakening the section in its details.

ROLLING TEMPERATURE.

The old types of rail in which the greater part of the metal was put into the head because it was the wearing surface and because it took the direct load for distribution to the other parts of the rail, left so little metal for the base that the edges were very thin and in the process of rolling they became much colder than the remainder of the section. This base was rolled at or near the critical temperature, resulting in a fine structure having great ductility; but at this temperature the metal in the base was so rigid that it prevented any work on the head. The result was that the head was finished too hot, giving a coarse structure which did not wear well and was brittle. To correct this, sections have been designed in which the size of the base is increased, making it possible to roll the entire rail at a more uniform temperature, but unfortunately robbing the head of some of its metal. However, the rolling of these rails so far has indicated that the structure of the metal in the head is much finer than in the old section and should give better results in track. None of these rails are yet in service so nothing is known as to how they will act under traffic.

The rolling of these rails has developed some surprises. It was expected that the rails could be rolled at a lower temperature than the old sections, and without thinking very deeply, most of us assumed that the shrinkage allowance could be reduced. We were therefore much surprised to find that under the same conditions, the new section would require a greater shrinkage allowance than the old. The rails were unquestionably rolled colder than the old section, with the exception of the thin flange, but it was this thin flange that determined the shrinkage of the old rails. In going through the cambering wheels the head was stretched giving the hot head a greater length for shrinkage than the base. In the new section the temperature is nearly uniform and much colder than the head of the old rail was, but no part of the new rails is as cold as the thin base of the old rail; consequently a greater shrinkage allowance is required.

THE STEEL.

A great deal is heard of open-hearth rails, and many persons seem to be impressed with the belief that the words "open-hearth" are a talisman which will cure all their troubles. Some rails which were made in open-hearth furnaces have given excellent service and other have given anything but good results. The writer some years ago suggested the use of very high carbon rails and advocated keeping the phosphorus as low as possible so that the carbon could be kept very high. In accordance with this the Pennsylvania Railroad made a trial lot of rails with 0.80 to 0.90 per cent. carbon and less than 0.02 per cent. phosphorus, the phosphorus being actually 0.009 and 0.011 per cent. These rails gave excellent service, not because they were open-hearth rails, but because the low percentage of phosphorus permitted us to use enough carbon to make them wear well. It is impossible to use this percentage of carbon in open-hearth steel much higher in phosphorus, and open-hearth steel much lower in carbon will not give the wear shown by these rails. On the strength of the behavior of these, 3,000 tons were ordered with 0.80 to 0.90 per cent. carbon and less than 0.03 per cent. phosphorus, and we then had an exhibition of some of the difficulties to be encountered with open-hearth steel.

In the converter it is easy to control the carbon content and hold it within narrow limits so that the ten points of allowable

variation were quite sufficient, but the open-hearth furnace cannot be controlled in the same way, and in making the 3,000 tons the heats varied in carbon by more nearly 20 points than 10, so that it ran about from 0.75 to 0.95 per cent. And since the amount of segregation of carbon increased with the carbon content, some of the steel was so brittle that it had to be rejected; and even then some brittle rails got into track. The rails as a whole gave very good results in track but it was their exceptional composition and not the mere fact that they were made in an open-hearth furnace which was responsible. This was shown clearly in the behavior of some open-hearth rails of 0.06 per cent. phosphorus in which we did not dare to run the carbon very high, and which in track did not wear as well as ordinary Bessemer rails. Unless we are able to get an exceedingly low phosphorus content in the basic open-hearth we had better hold to the acid Bessemer process.

Recently there has been much said and written on the seams occurring in the steel and considerable valuable work has been done, particularly at the Watertown Arsenal. Mr. Howard has reported on this and shown in detail that these seams occur in all the steel they have investigated, from the very expensive gun forgings down to the cheaper grades of steel. They are apparently as numerous in open-hearth steel as in Bessemer, in the bottom of the ingot as well as in the top, and there is no known method of prevention. This is a fact which we might as well look steadily in the face. Some method of prevention may be developed—will, in all probability, for the makers of steel are working hard to discover it—but at present it is a condition which we must consider and reckon with. This point does not seem to be as well understood as it should be. These seams are present in every rail made to-day; although they are relatively greater in some than in others, none are free from them. They are not defects in the sense of being the result of bad practice which could be corrected by the maker or detected by the inspector; it is a condition which must be considered in the designing of the rail. The knowledge that these minute seams are likely to be present should constitute the strongest argument for sufficient metal in the rail to compensate for the known deficiency in continuity. The statement has been made that all crescent-shaped breaks in the base are caused by these seams. This is a little too sweeping; the seams are not even

always present in the crescent breaks. The writer has had a great many breaks of this type which ran through seams but on the other hand he has seen many breaks of the same type in which the closest examination failed to show any seam—the break was entirely in solid metal.

In addition to the seams referred to above, some of the rails investigated at Watertown showed distinct stripes of ferrite or pure iron on the surface, running longitudinally along the rail. The stripes were of considerable width, probably $\frac{1}{16}$ in., and usually contained threads of manganese sulphide running through them. It has been suggested that the ferrite is developed by the manganese sulphide throwing the carbon out of solution, but this seems improbable since we know that sulphide of manganese exists to a considerable extent in the interior of the rails, particularly in the segregated portion, where there is no evidence of its producing any considerable areas of ferrite. The occurrence of these areas of ferrite on the surface seems peculiar and it seems likely that their cause will be found in something which affects the surface rather than in a substance like sulphide of manganese which may occur in any part of the rail.

We have lately been analyzing some of the heavy scales of slaggy material which adhere to the inside of the ingot molds and find them to be rich in oxide of iron and manganese. The patches of scale may be responsible for the ferrite streaks. The areas covered by scale have a much lower power of conducting heat than the clean iron mold; consequently there would be a lag in the cooling effect in these areas. The comparatively higher temperatures resulting from this lag would prevent the metal rich in carbon from solidifying because of its low freezing point; consequently the metal solidifying in these areas would be very low in carbon or possibly pure ferrite. Moreover, the oxide of iron in the scale would tend to reduce the carbon in the contiguous metal just as it does in the bath of an open-hearth furnace.

We have not been able to get any conclusive evidence bearing on this theory and it is mentioned as a possible cause of the ferrite streaks, which may be proved or disproved by future developments.

DISCARD.

In cropping the blooms at the shears, it is often impossible to tell when the piped portion has been entirely removed, and for a

number of years the feeling has been growing that to guard against piping a heavy discard should be made from the top of the ingot. The writer was in favor of this and advocated a discard of twenty-five per cent., believing that this was the only way to insure the removal of all the metal containing these internal defects. The objection to this was the waste involved. Of all the rails put in track by the Pennsylvania Railroad only about $\frac{1}{40}$ of one per cent. failed in any year. Assuming the average life at eight years this would be one-fifth of one per cent., so that in discarding 25 per cent. we would be discarding $24\frac{4}{5}$ per cent. of metal for the sake of eliminating one-fifth of one per cent. A test has been devised which makes this waste unnecessary; the same object is attained by the method of procedure advocated by the Pennsylvania Railroad and the American Railway Association and incorporated in the specifications which have recently been adopted by them. The crop end from the top of every ingot is tested to destruction, and if piping is shown, all the top rails from that heat are rejected. This practically means that about thirty per cent. will be discarded from all heats which show piping while those which do not show piping are cropped only the usual amount.

The Cambria Steel Company has rolled a considerable tonnage of rails under these specifications, and the testing to destruction unquestionably detected the pipes. To find to what depth the pipes extended we polished the ends of the drop test pieces and cut the top rail adjoining the test piece into small lengths, examining carefully each cut for pipes. It was found that of the heats showing pipes in the drop test piece when tested to destruction, sixty per cent. contained pipes so short that they were confined entirely to the crop end. Of the remaining 40 per cent. which extended into the top rail,

40	per cent. showed pipe extending more than	1	foot but less than	4	ft.
26	" " " " " " "	4	" " " " "	7	"
20	" " " " " " "	7	" " " " "	10	"
16	" " " " " " "	10	" " " " "	13	"
16	" " " " " " "	13	" " " " "	16	"
10	" " " " " " "	16	" " " " "	19	"
10	" " " " " " "	19	" " " " "	22	"
10	" " " " " " "	22	" " " " "	25	"
8	" " " " " " "	25	" " " " "	28	"

6 extended all the way through (very slight).

It will be seen that even under this specification 60 per cent. of the rails rejected contained no pipes. It would be quite feasible and legitimate to avoid this by the acceptance of all heats in which the pipe can be shown to be confined to the crop end. This can be determined by roughly polishing the end of the drop test piece.

DROP TEST.

The principal test for rails has for years been the drop test. When the sections were comparatively light, with low moments of inertia, and comparatively limber, and before the excessive rail wear caused by modern heavy wheel loads and dense traffic was so great a factor as it is to-day, the test worked very well. Very few rails broke under the drop, and the test was not questioned. But changing conditions have been so modifying the effect of the drop test that there is a question as to whether the test as we are applying it is a proper one. A 100-lb. rail will not stand as heavy a drop test as a 70-lb. rail of the same steel because it is stiffer; and as the wear caused by heavy traffic has necessitated harder steel with a higher carbon content, we are in a position to-day with our stiffer sections and harder steel where the drop test is causing the rejection of the very kind of steel we need in track. In a recent series of tests made on pieces cut from the same rail, some of which were subjected to 15-ft. fall and some to 19-ft. fall, in almost every case where a rail stood the 15-ft. fall but broke under the 19-ft. fall the steel showed plainly by its fracture that it was not brittle, but simply hard, and could not extend as fast as is necessary in the extension flange to avoid fracture.

The drop test so far exceeds any possible shock due to service conditions that it is unnecessarily severe and we must reduce it to avoid the rejection of any steel hard enough to give us the wear in the track that we should get. To show the relation between the blow of the drop test and service shocks we determined by trial the amount of fall required to strain the rail beyond its elastic limit and produce a permanent set. This was found to be about 12 ins. for 100-lb. and 8 ins. for 85-lb. rails. It is comparatively seldom that a rail is actually bent by the shock in service and yet we demand a test about twenty times as great in momentum and more than twenty times as great as far as the effect in the rail is concerned, as it is probable that the breakage under the drop is due

really more to the speed of the tup than to the momentum. In other words a test using a much heavier tup and a smaller drop might be fairer to the rail and more nearly approach the service conditions.

Moreover the drop test is simply a test of girder strength and, as pointed out earlier, 90 per cent. of the rail failures are not failures as girders but failures in the details.

We have many instances of steel which will bend under slowly applied stresses in such a way as to give the impression of extreme ductility, but which, under suddenly applied loads, is very brittle and will not stand any amount of distortion. Some impact test is necessary to detect this condition, but it should be a reasonable test, one which will differentiate between a strong hard steel and a brittle softer one.

Another element recently introduced into the drop test which has a great deal of influence on the problem is the requirement of a heavy anvil. For years the testing machines have varied greatly at the different plants, some having practically no anvil at all and very elastic foundations, while others were fairly rigid. The results were therefore not at all comparable. A fall of a given tup a specified distance had a very different effect in one machine than in others. To show the different results obtained by using different anvils we took pieces from the same rail and subjected them to the fall of a 2,000-lb. tup from the same height in the new machine at Sparrow's Point, in the axle-testing machine at Cambria and the rail-testing machine at Cambria. The Sparrow's Point machine has a 20,000-lb. anvil on a concrete foundation; the axle machine at Cambria has an anvil of 17,500 lbs., supported on springs; and the old Cambria rail-testing machine has about 3,000 lbs. of anvil. Taking the deflections obtained at Sparrow's Point as 100 per cent. those obtained in the axle machine were 97.6 per cent. and those in the Cambria rail machine were 75.3 per cent.

The manufacturers have recently adopted a standard testing machine having a 20,000 pound anvil supported on springs. This will eliminate the variations due to weight of anvil and character of foundation, and the results obtained with one machine can be directly compared with those obtained in any other. Moreover, a specified drop test will mean something definite and will not be, as Dr. Dudley has put it, a question of "geography."

This is only one of the many indications that the rail subject is being taken hold of in a scientific way. In other directions the same spirit is evident. For the first time we see signs of action by the railroads which will result in collection of accurate information regarding the behavior of the rails in tracks. The system of reporting on this subject advocated by the American Railway Association will unquestionably result in more accurate data than has been available before, and, taking the field as a whole, the indications are that we are in a fair way to get what in this subject in particular is badly needed, namely, evidence.

[For Discussion of this paper, see page 109.]

GENERAL DISCUSSION ON STEEL RAILS.*

THE PRESIDENT.—I would like to say that there is a sort of unwritten history that the Presidential Address shall not be made the subject of discussion. I am a most devout believer in fair play and I think it would be a great mistake for any man to get behind his office and say things that he would not say in the open. So I am going to say, for once at least, that the position that the Presidential Address is not to be discussed is waived, and I invite every one to tear it to pieces. What we want is the truth. If it comes by me I am glad; if it comes by you I am more glad. So please get your ammunition in order and be sure your powder is dry. **The President.**

MR. W. R. WEBSTER.—I want to take this occasion to thank the engineers and manufacturers who have coöperated with us in the work at Watertown Arsenal, and also to say that they have mapped out a very liberal program beginning with the ingot and ending with the finished rail. **Mr. Webster.**

This program will appear in the Proceedings. I wish to call attention to the important results obtained by Mr. Howard in connection with certain bending tests referred to in another paper. He took strips about $\frac{1}{16}$ in. thick from the top of the rail, and found that they could be bent, but that when bent crosswise they would split lengthwise, after they had been bent through only 25 to 30 degrees. I think this good evidence in favor of a deeper-headed rail.

* This discussion covers the following:

Report of Committee A on Standard Specifications for Iron and Steel, embodying Proposed Standard Specifications for Steel Rails.—Wm. R. Webster, Chairman.

Tests on the Metallurgy of Steel being Conducted at Watertown Arsenal, Mass.

Some Results of the Tests of Steel Rails in Progress at Watertown Arsenal.—J. E. Howard.

A Microscopic Investigation of Steel Rails: Manganese Sulphide as a Source of Danger.—Henry Fay.

Rail Failures, Mashed and Split Heads.—M. H. Wickhorst.

Some Notes on the Rail Situation.—E F. Kenney.

Mr. Snow.

MR. J. P. SNOW.—Mr. Fay's paper states the evils of manganese sulphide so strongly that perhaps I ought to modify some of the statements that I had intended to present.

I have heretofore maintained, and I still believe, that crescent breaks, split heads and shelly corners are due to manifest flaws in the rails as rolled. Manganese sulphide may be the cause of the flaws, but the flaws are evidently present in the steel before the fracture occurs.

I have here two hand specimens that I desire to bring to your particular attention. They were broken from the base of a rail in the laboratory. The rail had failed in service by a crescent break, but these pieces were at some considerable distance from the point of original failure. The larger specimen shows a seam which at one end is at the surface of the base, but at the other end leaves the surface and passes up into the metal and dies out. The plane of the seam is perpendicular to the base. Other seams parallel to this may be seen in the body of the metal looking as if the sides of the seams had been weakly stuck together previous to fracture. This class of seams I call gas-seams because they appear to occupy the same zone in the metal that gas-holes do in ingots.*

Many gas-holes occur quite near the surface, and if they open during the first passes in the blooming rolls they will naturally roll into seams similar to that on the edge of the larger specimen.

The smaller specimen shows another kind of flaw. It is in the surface of the base, does not pass up into the metal like the seam in the large specimen but remains its whole length on the surface and its plane is not perpendicular to the base like the other. Moreover its surface is not a plane but is fluted, and it has a more glistening appearance like mill-scale. I call this a rolling flaw, because I believe it is the result of a check or crack in the skin of the ingot or bloom. The crack leading to this type of seam may be due to manganese sulphide in the ingot. If an inclusion of this material existed very near the surface of an ingot it would be much weaker than the adjacent steel on account of its lag in solidifying, which, according to Mr. Fay is nearly 300 degrees Centigrade

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* "Experiments on the Segregation of Steel Ingots," etc., by C. L. Huston, *Proceedings*, 1906, p. 182, and paper on "Piping and Segregation in Steel Ingots," *Bi-monthly Bulletin Am. Inst. Mining Engineers*, No. 14, March, 1907, p. 202.

Mr. Snow.

If cracks occur from shrinkage, handling, or severe reduction in the rolls, they will naturally occur at the weakest points; hence it is not unnatural that manganese sulphide should be found at these seams. The fact remains, however, that the seams are there; some very minute, some easily visible on a new rail as a distinct mark several feet long. Captain Hunt told us at last year's Convention how blooms were torn in the mill.* Such cracks can result only in seams in the finished rail such as I here call rolling flaws. The smooth heat-finished surface of the walls of these seams proves to my mind that they were complete fissures when they left the rolls.

Flaws of this class are not so numerous as gas-seams, in the rails that I have been privileged to examine, but my observation leads me to think that they are the cause of a great majority of the flange breaks that have been so numerous in rails manufactured during the last decade.

Mr. Fay has just stated that seams of manganese sulphide are the cause of these breaks rather than rolling flaws. It seems to me that we may both be correct. The faces of the seams that I have almost invariably been able to locate as the origin of these breaks, are smooth and by no possible construction classifiable as fractures of sulphide or anything else. They show the ear-marks of heat while still in the rolls. Manganese sulphide may be the cause of the crack in the ingot or bloom but the fissure in the rail causes the breaks.

I have a third specimen for your inspection that illustrates the split head proposition. I am glad to say it coincides with what Mr. Wickhorst has told us at this meeting and with Dr. P. H. Dudley's very instructive paper of last year.† The black material located about $\frac{1}{8}$ in. below the wearing surface of the head is very apparent. It may be oxide or some other salt. It certainly is not steel. It is obviously the starting point of the fracture as illustrated, and explained by Dr. Dudley in the paper above referred to. The split in the head of which this formed a part was about 10 ft. long and was thought at first to be a pipe, but I think now that it was a progressive fracture induced by the wedging action of the soft mass of included salts.

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* *Proceedings*, 1907, p. 96.

† *Proceedings*, 1907, p. 54.

Mr. Snow. Of the three classes of defects named above, gas-seams, rolling flaws and inclusions of manganese salts, the two latter are, I think, by far the most dangerous; and, while it is probably impossible to entirely prevent their occurrence it is possible to greatly lessen their number and decrease their efficiency for evil. Mr. Howard has shown us that he finds evidence of unsoundness in some degree in all the rails that he has examined. Some of these rails have, nevertheless, done magnificent service. This demonstrates that when conditions are favorable, either by accident or by intelligent control, the defects unavoidable in rails made from melt-made steel can be tolerated without undue risk.

If gas-holes are deep seated in the ingot so that the resulting seams will be from $\frac{1}{8}$ to $\frac{1}{4}$ in. inside the surface of the finished rail they do not seem to be of vital damage. Mr. E. von Maltitz* has shown that if the steel is not overblown, and the deoxidizing thoroughly done, and the metal teemed at a sufficiently low temperature, the gas-holes will be few and deep-seated. If the spiegel is added in the vessel where all parts of the bath are hot, and if enough time is allowed for the reactions to become complete and the resulting salts to rise into the cinder before the metal is teemed into the molds, gas-holes will not only be reduced, but inclusions of foreign salts will be practically absent. If now the reductions in the blooming mill are carefully made the principal source of rolling flaws will be eliminated.

Mr. Fay recommends an interval of time between the addition of the ferro-manganese and teeming to the molds, and Mr. P. H. Dudley is actually enforcing this as a specification stipulation. He writes me from the Illinois Steel Company, South Works:

> Restricting the ingots to three-rail lengths and holding the steel three minutes after recarburizing, in connection with the dry blast at South Chicago shows a marked reduction in seams and cracks in the bases of rails. In a lot of 2,500 tons of rails last Friday and Saturday hardly a trace of seam has been found in any of the rails.

The dry blast referred to is the Gayley process of furnishing air, practically free from aqueous vapor, to the converters while blowing the charge. This decreases the amount of iron oxide in

* In a paper on "Blow-Holes in Steel Ingots," read before the American Institute of Mining Engineers at the Toronto meeting, July, 1907.

the bath which Von Maltitz claims to be a principal agent in producing blow-holes. Mr. Snow.

Where possible the recarburizing agent should be added in the converter after it is turned down. In this condition the bath and walls of the vessel are hot and all parts of the metal can be acted upon promptly by the reagent. It stands to reason that if the spiegel is added to the more or less cooled bath in the ladle that the reactions will be sluggish and that nodules of the resulting salts will be caught in the solidifying mass in the ingot and when rolled will be drawn into seams such as we see in the finished rail. The principal objection to adding the spiegel to the steel in the converter is, that it requires more of it, as part of the manganese is absorbed by the cinder lying on the bath in the vessel.

A fourth type of unsoundness is due to segregation. This leads to crushing of the head, flow of metal and rapid wear, rather than to absolute breakage. It can be controlled in some degree, according to Professor Howe, but it is inevitable to a greater or less extent when steel passes from a liquid to a solid state. Segregated material in the head of a rail, especially if near the surface, is very objectionable, but if confined to the base will lead to but little trouble. Two ways of attaining this end are open, first by throwing the ingots down before freezing is complete and controlling the turns so that the top of the ingot, as it lies in the soaking furnace, shall be formed into the base of the rail; and second by rolling the ingot to a slab about 7 x 15 ins., and splitting it at the last pass by cutting disks or otherwise, and rolling each biliet into rails in such a way that the base shall be formed from what was the central part of the slab. These methods would keep the segregated metal out of the head where the blows of the wheels render its presence very undesirable.

In all the failed rails that I have examined, the steel, where sound, is excellent. Mr. Howard's investigations lead to the same conclusion too, I think. The deleterious effects of improper heat treatment, *after* the metal has solidified, do not seem to be in evidence to an important extent. In fact, I believe that our recent requirements of shrinkage for heavy rails of the A. S. C.E. pattern are inconsistent and all wrong. It is unsound steel that we should guard against.

The exhibitions of Mr. Howard, Mr. Fay and others and the

Mr. Snow. experience of Mr. P. H. Dudley, Mr. Robert Job, Mr. R. W. Hunt, Mr. M. H. Wickhorst and many others, constitute a remarkable indictment of the methods of rail manufacture during the past decade or longer. The way seems plain to better results. Will our manufacturers come forward to relieve the situation? Mr. Job said before our meeting in 1905:

> The results of our investigation indicate that the greater part of the difficulty which occurs to-day with rails under heavy traffic is due to the unsound condition of the steel; a condition which existed in comparatively slight degree in the earlier rails.

Our investigation and studies since this was written have but served to intensify Mr. Job's statement by classifying and identifying the various sorts of unsoundness.

I believe the investigations that Mr. Howard has started should go on. We need examinations of rails rolled under rational regulations of the recarburizing process, as for instance those made under Mr. P. H. Dudley's specification. We want examinations of open-hearth rails; we need to have the imperfections of each of these classes compared with those of the ordinary Bessemer rail of to-day, and we want all kinds of rails available, tested for rolling flaws. We want the scoring of the base, that results from the higher speed of the edge of the roll over that of the rail, investigated to see if its effect is deep-seated. We want the effects of the straightening gag fully explored; and we want these features photographed, discussed and correlated, so that they can be fully understood. We want blooms and cobbles examined to find, if possible, the origin of these various defects, and we want ingots opened up in order to study the various phenomena of cooling. Especially we should have a quarter of an ingot opened on the diagonal to show the conditions of columnar crystals and entangled gas-holes at the corner where crystals from two sides interfere. The difference in this feature between ingots with square and well rounded corners should be observed.

It is possible that shelly corners of the rail may be due in large measure to the conditions caused by the corners of ingots being too nearly square. We want the chemistry of the streaks and segregated portions thoroughly examined and we want all this done by disinterested and unbiased parties, who can draw con-

clusions that may lead to improved manufacture as well as to means of testing, so that consumers may test for soundness in addition to the present test for absence of brittleness. **Mr. Snow.**

Mr. Henry Fay.—The idea as to manganese sulphide is not that it will explain all cracks and breaks. My point is that it is a harmful substance. If you predict that a crack will occur in a certain place and then subject that metal to strain and produce a crack through a particular area, then you have reached a point where you have proven almost absolutely that manganese sulphide is a dangerous element. All I wish to state now is that it is something that we ought to avoid, inasmuch as the process of avoiding it is so very simple. **Mr. Fay.**

Mr. R. W. Hunt.—To you, Mr. President, a discussion on steel rails must be particularly interesting, recalling to you, as it must, an event which I may be permitted to designate as one of your earliest professional triumphs. While I did not at that time agree with all of your deductions, nor do I now think they were all correct, I do know that you were about the first scientist bold enough to tell the steel rail makers, of whom I was then one, that they did not know how to, or else would not always, make steel best suited for rails. **Mr. Hunt.**

To me as to you the present renaissance of steel rail making must bring up many points with which we have been for years familiar. In such epochs there is always danger that while seeking improvement time will be wasted in threshing over old straw, which loss could be avoided by a little research as to the practices and results of the past. Because a detail of manufacturing has been abandoned, it does not at all follow that such action was taken because that detail was not a good thing.

In the current discussions on steel rails, much has been presented as newly-discovered facts, that has been known for years but ignored or even denied, largely because its importance was under-estimated; and I believe that in many cases this was the controlling reason, more readily made controlling because recognition consumed time and thus, and in other ways, increased the cost of production.

It has long been known that better, that is, sounder, castings or ingots (an ingot is a casting) can be made by pouring the steel through comparatively small nozzles; also that short ingots roll

Mr. Hunt. better, that is, yield more perfect blooms, than longer ones of the same cross section. Such observations came in my experience as far back as 1871, as when at Cambria we changed from rolling 60-lb. rails to 50-lb. ones, rolled from shorter ingots cast in the same molds; in both cases each ingot contained steel enough for two 30-ft. rails. The steel from the shorter ingots almost invariably rolled best.

Another well known thing is that what are technically known as "butts," and which are short ingots, produced through being the last one poured of a blow of steel, and short because of there not being sufficient metal to make a full length ingot, generally roll better than the longer ingots of the same heat.

That time should be given for the necessary chemical reaction after the addition of the re-carbonizer before casting the metal into ingots, has been known for at least thirty years. It was known that such time was also important for the escape of the occluded gases, and the value of this latter knowledge was manifested by the several devices for accelerating such escape which, years ago, were either proposed or actually used. These ranged from the thrusting into the ladle full of molten steel a wooden pole, or placing in the ladle before it received the steel from the converter, pieces of wood saturated with kerosene, to more elaborate devices for agitating the steel while in the casting ladle by power-driven, refractorily-protected screws. It was the practice at some of the Bessemer works to again put on the blast after the introduction of the re-carbonizer, and then to partially turn the vessel up, thus agitating the charge.

Mr. Robert Forsyth sought to accomplish the desired results through his transferring ladle arrangement, by which the ladle after receiving the steel from the converter was transferred by a hydraulic ram from the receiving crane, to the ladle or casting crane. This he did when remodeling the Union Steel plant, Chicago, about 1886. Later he put the same arrangement in the South Works of the Illinois Steel Company, after becoming chief engineer of that company, thus again assuming command of a plant originally designed and built by him. Later Mr. Wm. R. Walker carried this further by pouring the steel over the top of the receiving ladle into the casting ladle through a nozzle in the bottom of which it was cast in the usual way.

That mechanical defects would be developed to a greater or

less extent by the rolling process, was recognized, and therefore the bloom, about 7 ins. square, was conveyed from the blooming rolls to a steam hammer by which all visible cracks or defects were chipped out, care being taken to cut to the bottom of the imperfection, and not leave any pronounced shoulders at the edges of the resulting depressions; and until the adoption of automatically operated tables attached to the rail rolls, if the partially formed rails still showed defects, the operation of rolling was halted, while such places were chipped out by hand. **Mr. Hunt.**

The foregoing were usual practices, and were not abandoned because of their results being unsatisfactory, but on account of the time consumed and the expense incurred. The pressure of the commercial race forced the technical steel maker to try to convince himself that such refinements of manufacture were unnecessary.

Personally, I have been proud of having, in about 1873, after leaving Cambria and taking charge of the Troy Works, participated in starting the race for increased output. Fry, Forsyth, Captain Jones, and later many others were equally eager, and from that time on the race has continued a merry one. I have also been proud of having put in the first automatic railmill tables. Nevertheless, I sometimes wonder how well I built. But I was not alone, and I guess the history had to be made, so one individuality made but little difference.

Another point on which, in view of somewhat recent statements, there is danger of misapprehension, and consequently mistakes, relates to the treatment of the ingots between their casting and rolling. The endeavor to utilize the initial heat of the ingots dates in this country from the early '70's; in fact, the Cambria plant started in 1871 with that practice.

That blooming mill had gas heating furnaces which, at first, did not always work satisfactorily. I had charge of the converting works, John E. Fry of the blooming mill, and Captain Wm. R. Jones was the assistant chief engineer, and so a general free lance as to criticism. We were all three good friends and so each frequently used all of a friend's privileges. I recall that one day Fry's furnaces were working particularly badly, and Jones made himself agreeable(?) by suggesting to him that he should request me to hurry up the delivery of the freshly cast ingots so that his furnaces could have the benefit of their heat; thus anticipating to some

Mr. Hunt. extent one of the principles of the Gjers soaking pits, and leading to the invention, and permanent adoption, of the Hainesworth gas-fired pit or furnace. In both cases recognition was given to the necessity of keeping the ingots in an upright position until their interior steel had solidified.

As is well known, the old practice was to cast the ingots in molds standing in a pit, later stripping off the molds, and then placing the ingots horizontally across iron cars, on which they were conveyed to horizontal heating furnaces into which they were charged. This procedure consumed time, so the thought was to in some way shorten it. Captain Jones, at the Edgar Thomson plant, in either the latter part of 1887 or early in 1888, changed from the above, by picking up both the mold and its contained ingot and laying them across the cars, which were then pulled outside of the converting house and before a hydraulic ram, the piston of which forced each ingot in its turn out of the mold and onto another car placed to receive it. The ingots were then taken to and charged into horizontal furnaces.

Captain Jones had more thoroughly, and at much greater expense, than any other American engineer, endeavored to make the Gjers non-fired soaking pit work, but was unsuccessful. Natural gas had been piped to Pittsburgh, and the Hainesworth vertically-fired pit or furnace was developed by William Hainesworth at the Pittsburgh Steel Casting Company's Works. It was heavy and hot labor to charge, properly turn during the heating, and then draw the ingots from the old style furnaces, which labor had been greatly augmented by the increased weight of the ingots. The vertical furnace either minimized or abolished such labor; therefore, they were put in at the Edgar Thomson plant.

I know that Captain Jones did not want to believe that the placing of the ingots in a horizontal position so soon after casting increased the tendency to piping, but I have reason to think he was so convinced. At all events, in 1892, the procedure was abandoned, and the present practice of casting on cars and keeping the ingots in a vertical position was adopted. L. G. Laureau had advocated this manner of operation in a paper read before the American Institute of Mining Engineers in February, 1885. But it must be remembered that if the time elapsing between casting the ingot and placing it in a horizontal position on the tables of the

blooming mill has not been sufficient for the interior steel to solidify, even though the ingot has been in a heating furnace, the steel will flow and interior defects will be produced, which, in some form, will be found in the resulting rails; and perhaps the damage may not be in the rail made from the top bloom. It should be remembered that the piping of ingots is not a new discovery, but, on the contrary, has been well known for years. Robert Forsyth at the Union Steel Works in 1888 demonstrated the relation of the length of the pipe to the position of the ingot while its interior metal was solidifying, by breaking a number of ingots which had been differently handled—some placed in a horizontal position as soon as possible after being cast, and others so placed at varying intervals up to having been kept vertically until all of the steel was thoroughly set. As a matter of reference, I would mention that I gave his results together with photographs of the broken ingots in a paper contributed to The Franklin Institute, January 21, 1889. **Mr. Hunt.**

I call attention to the foregoing facts simply to show that the unsatisfactory experiences with rails during the past few years have not all come from newly-discovered causes, nor were the means by which such undesirable qualities could be avoided unknown—they had simply been ignored. Therefore, the science of obtaining a comparatively sound ingot, and making from it a good and safe rail, is not an unknown or mysterious one. I do not mean that there cannot be anything more learned about steel; far from it, and such researches as outlined in the paper just presented by Professor Fay are most valuable. The alloys of the rarer metals may give satisfactory results, but in the meantime we need not despair.

A rail of heavy section will probably never be as good as one of lighter section made from the same metal, the impossibility of giving it the same work will always affect the result; and it is not likely that we will ever return to all of "the practices of the Fathers." But there are not sufficiently good reasons for ignoring so many of them, and I am glad to say that several rail makers are returning to some of the old ways. They are taking more time in casting, using smaller nozzles, casting shorter ingots, and, of course, a shorter ingot will permit of less taper in the mold; and I believe they are finding themselves rewarded by a better output—

Mr. Hunt. at least a smaller percentage of seconds, that is, rails with manifest defects.

The Gayley refrigerated blast is also being tried. When used on blast furnaces, it seems to produce a better, or at least a more uniform quality of iron, and good iron is certainly a most important factor in making good steel. The results obtained from the dried blast when used in the Bessemer converter will be an interesting metallurgical development.

The practices of both rail users and rail makers have been quite freely discussed, and I believe both are willing to admit some faults, so I believe we may have much hope for the future.

Mr. Tiemann. MR. H. D. TIEMANN.—May I present a point of view which I think has been overlooked, as to the mechanical relation between the rail and the wheel? In regard to the nature of the rolling contact, when you come to analyze it to see exactly what takes place when one body rolls upon another, you will find, I think, that it must necessarily consist of a series of vibrations or successive impacts. That is well seen if you take a bicycle, the tire of which is deflated, and run it along on a smooth pavement. It will indicate on the pavement a series of spots, showing that the rolling is not continuous, but that it consists of a series of impacts.

Now in the case of impact between two bodies there are two distinct things that happen; one being the impact between the two bodies as a whole, producing a force which is simply proportional to the rate of change in the momentums of the two bodies, and the other being the local action upon the material immediately at the point of contact, which material acts as a cushion between those two bodies at the point of contact. This latter is the important action to which I refer and wish to bring out. Now in the case of the wheel on the rail this local impact is solely between the wheel and the rail, and is entirely distinct from the impact that occurs between the train and the roadbed. These are two entirely distinct features. For instance, if you make an impact test on a beam supported on an anvil, the local indentation which will occur at the point where the hammer strikes the beam is due entirely to the inertia of the beam; it is not due to the anvil upon which it rests. Were the beam mass-less, no indentation would occur. So in the case of the wheel upon the rail, the effect which necessarily follows is a series of local compressions which are due simply to

the impact of the weight, or rather mass, of the wheel, and the mass of the rail, and are independent of the roadbed or the weight of the train. Let me be clearly understood that this local effect to which I refer is in addition to that of the simple impact between the train as a whole and the track or roadbed. Now this being the case, increasing the weight of the rail and increasing the weight of the wheel itself will, of course, necessarily increase the effect of that impact instead of correcting it. Increasing the weight of the rail and wheel, would, of course, increase the strength of the rail acting as a beam in resisting the effect of the impact of the two bodies as a whole. But we have just the opposite effect on this local compression which is due, as explained, to the inertia of the local portion of the mass of the rail itself and to that of the wheel. This must necessarily be one cause, and probably the chief one, of the flattening out and scaling of the upper surface of the rail, recently described. Hammering a long time, as stated by a former speaker, will produce the same effect, and for precisely the same reason. **Mr. Tiemann.**

Now this may be an old story. If it is I hope you will excuse me for presenting it. But I have never seen this matter brought out clearly. The only way I can see of correcting anything of this kind would be by taking into consideration some design which would in some way lighten the mass of the wheel or the mass of the rail, at the same time keeping the strength up, or by using a material at the immediate surface of contact which had a greater elastic resilience.

This effect of local action is really very much greater, I think, than one would imagine until he study into it fundamentally. The local compressive force set up by this impact at the point of contact between a wheel and a rail, or between a hammer and a bar of metal, may reach a prodigous amount, far in excess of the force of impact between the two bodies themselves, taken as a whole, and it commonly exceeds the ultimate crushing strength of the metal. Hence the resulting injury by flattening of the surface.

Mr. Onderdonk. MR. J. R. ONDERDONK.—I would just like to say a few words in regard to rails, from the standpoint of the railroad. A few years ago we began to feel that all the rails were poor, some of them better than others. On the Baltimore and Ohio Railroad the principal trouble with the rail has been the wear and breakage

Mr. Onderdonk. due to slipping of drivers, the split head, and lately the corrugated rail. The split head has given us more concern than any other form of failure. About three years ago the split head question became so serious that, out of 10,000 tons rolled and put in the track, 22 per cent. were removed during the first year, on account of depressions in the head. When broken apart, the majority of them showed this opening in the head. The percentage of crop from the ingots from which these rails were rolled, varied from 3 to 5. When the head split on the gauge side it was liable to cause derailment, and the matter was so serious that it was suggested that 30 per cent. would be cropped from the ingot. For several years, that percentage was cut from the top of the ingot. The rails rolled by the same concern, cropping 30 instead of 5 per cent., and placed in the same division, with practically the same traffic over it, reduced the percentage of split heads from 22 to $1\frac{3}{4}$, during the first year, although the 30 per cent. discard did not begin at the first of the year. The split heads were reduced to about 0.6 per cent. for the second year's rolling, showing that the cropping of the ingot prevented, to a large extent, the split heads. The burning of the head of the rail, due to the spinning of drivers, has caused some broken rails; but a rail broken into two or more pieces is a rare occurrence. The burnt rail usually shows a crack extending down into the head, and in some cases as much as three-fourths through the head.

The moon-shaped breaks spoken of do not occur on this road, unless the track man misses the head of the spike and strikes the flange.

Rails with split heads cause the greatest concern, and to overcome this difficulty the Baltimore and Ohio Railroad issued on March 1, 1908, their specification No. 163-B for steel rail, which, in Section 3, provides for a test and inspection to detect this defect, and, we think, gives the inspector a better opportunity to do so than some of the other specifications.

Mr. Stevenson. MR. A. A. STEVENSON.—I do not know that I have much to say about rails, but I cannot get away from the steel tire question. The photographs I am going to show are simply to illustrate the similarity of conditions that exist in rails and tires. I have three photographs here of treads of certain tires which have already been passed off on one or two rail makers as photographs of

the surface of rails (Figs. 1, 2 and 3). They correspond almost exactly to the condition of rail surfaces that are found at stations, **Mr. Stevenson.**

FIG. 1

FIG. 2.

especially where the locomotive starts the train. There is considerable heat produced by the fact that the treads of the driving wheels are moving at a considerably higher rate of speed than the

Mr. Stevenson.

FIG. 3.

FIG. 4.

train. There is produced then on the rail a hard, slip or skid spot. When the condition is reversed and the train is moving faster than the treads of the wheels a slip or skid spot is produced on the tread of the wheel or tire. The wheel is then in an incipient stage of **Mr. Stevenson.**

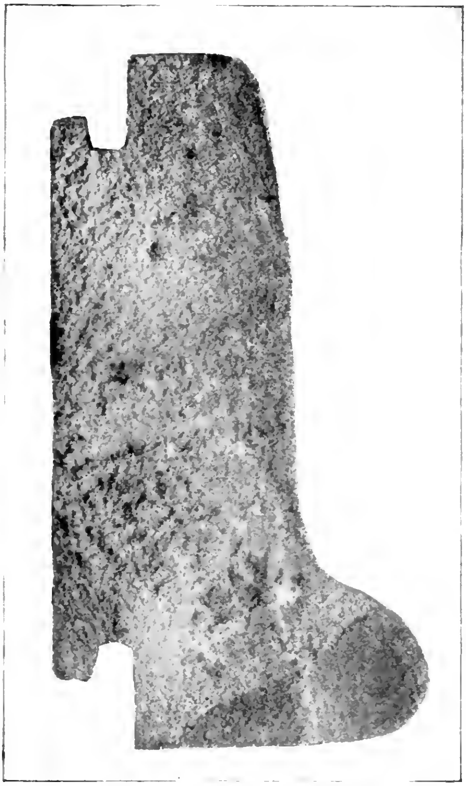


FIG. 5

eccentricity with hard spots on the tread, and as a result is very apt to become shelly.

There are also the heat cracks on the point of the flange, such as exist on the corner of a rail head. I have here three photographs which show in a very pronounced manner these heat cracks from which the fracture originated. There are certain conditions

Mr. Stevenson. which produce these cracks, and while the suggestion has been made that there may be incipient cracks in the steel, yet in the examination of hundreds, I might almost say thousands, of finished tires we have never found any evidence of such cracks.

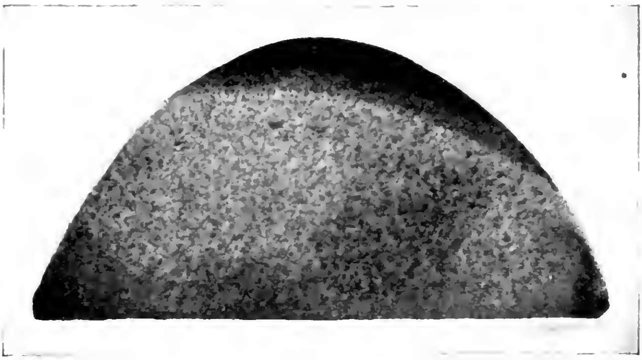


FIG. 6.

It hardly seems possible to us that we should inadvertently concentrate over 75 per cent. of the steel that had incipient cracks on orders representing only about 4 to 5 per cent. of our output.

Fig. 4 shows the incipient cracks and the heat cracks from which they originate.

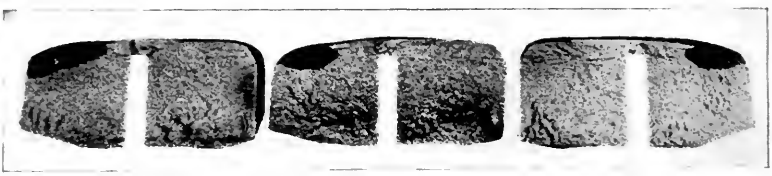


FIG. 7.

Fig. 5 is a broken section of a tire and shows very plainly the manner in which the fracture occurred, the origin of the crack being a heat crack on the point of the flange.

Fig. 6 shows part of a flange of a driving tire enlarged two diameters. The depth of the heat crack is clearly shown. **Mr. Stevenson.**

Fig. 8.

Fig. 7 shows three sections of a broken rail; fracture due to cracks on corner of the heat. Compare with Fig. 5. Fig. 7 is a reproduction of an illustration used by Mr. Howard.

Fig. 8 shows a section through a slip spot, natural size.

SOME RESULTS SHOWING THE BEHAVIOR OF RAILS UNDER THE DROP TEST, AND PROPOSED NEW FORM OF STANDARD DROP-TESTING MACHINE.

By Simon S. Martin.

From the very prominent place given drop tests in rail specifications, it might seem that the behavior of the rail under the drop test given is generally regarded as valuable information as to its character. As a matter of fact, however, engineers differ widely as to the advisability of accepting this test as an index to the reliability of the rail on account of the great variation in the results obtained by this test. The test shows, it is true, whether the piece being tested is brittle or not, and by observation of the permanent set, whether the steel is soft or hard. The object of this paper is to give some results obtained.

Average specifications, which a majority of the railroads have in recent years used as a standard, contain the following clause as to drop test:

One test shall be made on a piece of rail, not less than 4 ft., nor more than 6 ft. selected from each blow of steel. The test piece shall be taken from the top of the ingot. The rails shall be placed head upwards on the supports, and the various sections shall be subjected to the following impact tests under free falling weight:

70 to 79-lb. rail.	18 ft. drop.
80 " 89 " "	20 " "
90 " 100 " "	22 " "

If any rail breaks, when subjected to the drop test, two additional tests may be made of other rails from the same blow of steel, also taken from the top of the ingot, and if either of these latter rails fail, all the rails of that blow which they represent will be rejected; but if both these additional test pieces meet the requirements, all the rails of the blow which they represent will be accepted.

The drop-testing machine shall have a tup of 2,000 lbs. weight, the striking face of which shall have a radius of not more than 5 ins. and the test rail shall be placed head upward on solid supports 3 ft. apart. The

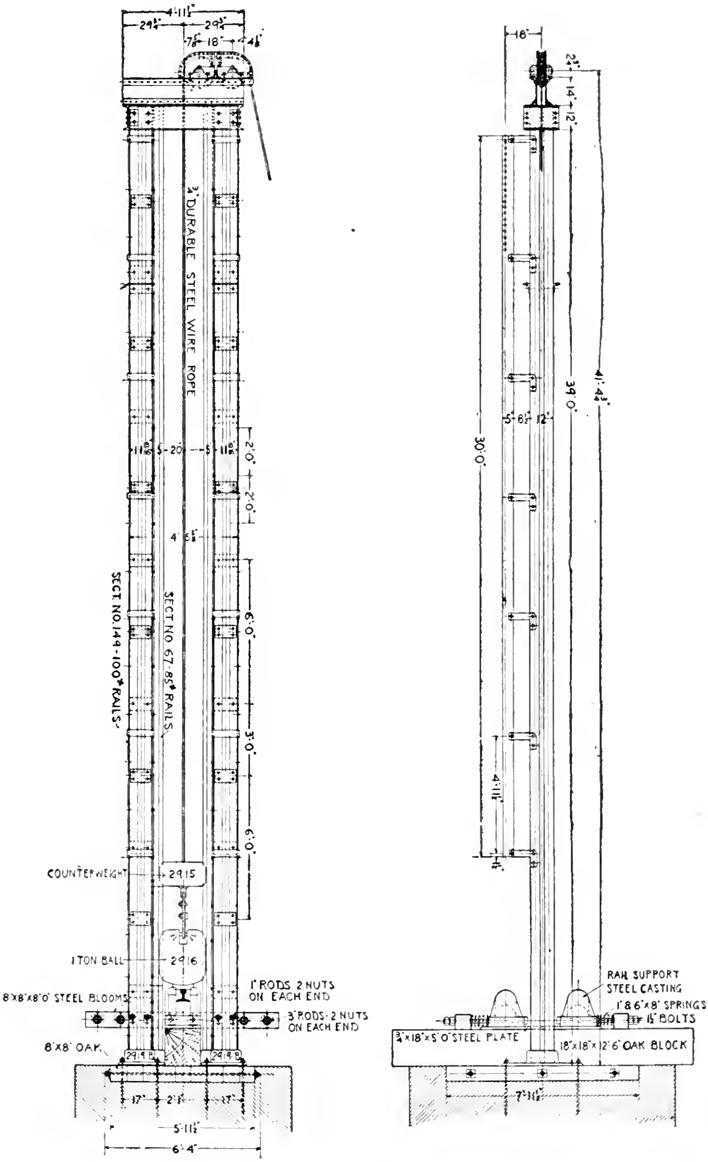


Fig. 1.—Drop-Testing Machine, Type "A."

anvil block shall weigh at least 20,000 lbs., and the supports shall be part of or firmly secured to the anvil. The report of the drop test shall state the atmospheric temperature at the time the test was made."

These specifications, while used by the railroads, have to be modified according to the character of the drop-testing machines at the different mills. Thus, we find machines answering closely the following descriptions:

1. A drop-test machine consisting of some concrete and loose stone, supporting a number of 12 x 12 in. oak ties, 12 ft. long, on which is placed an oak block 18 x 18 ins. x 11 ft. On the oak

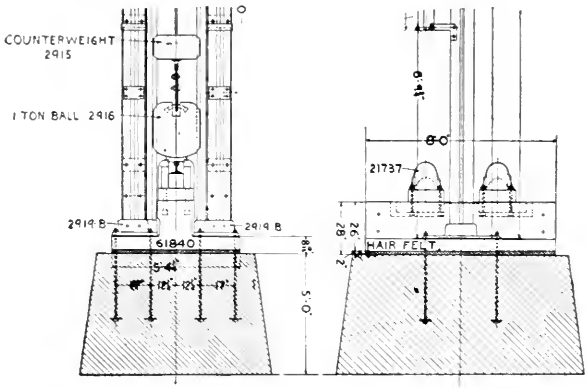


FIG. 2.—Base of Drop-Testing Machine, Type "C."
Upper portion like Type "A."

block are two steel plates 1 x 18 ins x 7 ft., which become the bearings for the rail supports. These supports weigh 1,300 lbs. This we shall call Type "A" Machine (Fig. 1).

2. A drop-test machine consisting of a wooden foundation 4 ft. deep and 10 x 10 ft., on which were placed two blooms, probably 8 x 10 ins. x 10 ft. On the blooms are placed the rail supports. This we shall call Type "B" (not illustrated).

3. A drop-test machine consisting of a concrete or stone foundation, on which rests a 20,000-lb. anvil, to which the rail supports are securely fastened. This we shall call Type "C" (Fig. 2).

There are probably other types with which the writer is not familiar, but these suffice to show their great variation, and em-

TABLE I.—RESULTS OF TESTS ON TYPE "A" MACHINE.

Test pieces taken from same position in ingot. Separate pieces used for each height. 2,000-lb. ball; 3-ft. span. Temperature, 40° F. January 8, 1908.

Drop. Ins.	85-lb. Penna. R. R. Permanent Set. Ins.	85-lb. Penna. R. R. Recovered Deflection. Ins.	85-lb. Penna. R. R. (New Section.) Permanent Set. Ins.	85-lb. Penna. R. R. (New Section.) Recovered Deflection. Ins.
6	.0	.187	.0	.200
12	.031	.312	.0	.250
18	.093	.250	.062	.281
24	.125	.312	.093	.312
36	.250	.281	.171	.343
48	.343	.312	.250	.281
60	.406	.343	.343	.312
72	.513	.312	.468	.312
84	.700	.343	.600	.375
96	.800	.375	.700	.343
108	.900	.375	.800	.343
120	1.00	406	.850	.375
	100-lb. Penna. R. R.		100-lb. Penna. R. R. (New Section.)	
6	.0	.109	.0	.109
12	.028	.218	.0	.250
18	.082	.312	.031	.375
24	.125	.312	.093	.281
36	.218	.312	.109	.281
48	.281	.343	.250	.406
60	.375	.281	.312	.312
72	.437	.281	.375	.375
84	.650	.281	.375	.281
96	.700	.281	.600	.281
108	.750	.312	.700	.500
120	.800	.343	.800	.343

phasize the need of a standard drop-testing machine from which truly comparative results may be expected.

To ascertain how the results of drop tests are affected by the type of machine used, the following series of tests were made:

Definition of Terms.—Set is the permanent deflection.

Recovered deflection is the rebound, and is obtained by placing a block of soft wood under the rail into which block a small spring-steel rod is inserted, which just touches the base of the rail. The distance this rod penetrates the wood is the *total* deflec-

TABLE II.—RESULTS OF TESTS ON TYPE "C" MACHINE.

20,000-lb. anvil. Temperature, 45° F. January 18, 1908.

	85-lb. Penna. R. R.		85-lb. Penna. R. R. (New Section.)	
Drop. Ins.	Permanent Set. Ins.	Recovered Deflection. Ins	Permanent Set. Ins.	Recovered Deflection. Ins.
6	.03	.16	.00	.06
12	.16	.16	.06	.09
18	.17	.13	.14	.13
24	.25	.16	.22	.08
36	.38	.16	.40	.08
48	.50	.17	.55	.16
60	.68	.16	.70	.16
72	.80	.17	.80	.16
84	1.00	.22	1.00	.16
96	1.10	.22	1.10	.22
108	1.28	.19	1.25	.22
120	1.35	.28	1.42	.22
	100-lb. Penna. R. R.		100-lb. Penna. R. R. (New Section.)	
6	.0	.09	.0	.19
12	.05	.13	.06	.03
18	.08	.16	.13	.09
24	.08	.16	.16	.09
36	.31	.17	.31	.09
48	.50	.13	.38	.09
60	.70	.16	.50	.08
72	.80	.16	.70	.13
84	.90	.17	.75	.16
96	1.00	.17	.90	.13
108	1.30	.17	1.05	.11
120	1.50	.17	1.10	.11

tion, and the *recovered* deflection is measured from the end of the rod to the base of the rail.

From the results on Type "A" Machine (Table I) it is seen that for the 85-lb. rail section a drop of 12 ins. produced a permanent set of .031 in., while on the new section, with thick flange, a 12-in. drop did not give a permanent set, although at 18 ins. a permanent set of .062 in. was produced.

From the results of tests on Type "C" Machine (Table II) it is seen that a drop of 6 ins. gives a permanent set of 0.03 in. on P. R. R. 85-lb. rail, and on the 85-lb. new section a drop of 12 ins. gives a set of 0.06 in., while on 100-lb. P. R. R. rail a 12-in.

TABLE III.—RESULTS OF DROP TESTS OF RAILS TAKEN FROM DIFFERENT POSITIONS IN INGOT.

100-lb. rail, Penna. R.R., new section. 2,000-lb. ball. 3-ft. span. 19-ft. drop.

	Permanent Set. Ins.		Permanent Set. Ins.		Permanent Set. Ins.
A–T–1	1.00	A–M–1	Broke	A–B–1	.95
A–T–2	1.05	A–M–2	Broke	A–B–2	Broke
A–T–3	1.10	A–M–3	1.00	A–B–3	Broke
A–T–4	1.10	A–M–4	1.00	A–B–4	Broke
A–T–5	Broke	A–M–5	1.10	A–B–5	Broke
A–T–6	Broke	A–M–6	1.05	A–B–6	.95
B–T–1	Broke	B–M–1	.90	B–B–1	.90
B–T–2	1.10	B–M–2	1.00	B–B–2	1.00
B–T–3	1.15	B–M–3	1.10	B–B–3	Broke
B–T–4	1.20	B–M–4	1.15	B–B–4	1.00
B–T–5	1.30	B–M–5	1.10	B–B–5	1.15
B–T–6	1.20	B–M–6	1.00	B–B–6	1.15
C–T–1	1.10	C–M–1	1.05	C–B–1	1.00
C–T–2	1.20	C–M–2	1.10	C–B–2	1.00
C–T–3	1.10	C–M–3	1.00	C–B–3	1.05
C–T–4	1.00	C–M–4	1.10	C–B–4	Broke
C–T–5	1.20	C–M–5	1.10	C–B–5	1.05
C–T–6	1.10	C–M–6	1.00	C–B–6	1.00
D–T–1	Broke	D–M–1	1.20	D–B–1	1.20
D–T–2	1.40	D–M–2	1.20	D–B–2	1.20
D–T–3	1.40	D–M–3	1.30	D–B–3	1.30
D–T–4	1.40	D–M–4	1.30	D–B–4	1.30
D–T–5	1.40	D–M–5	1.30	D–B–5	1.20
D–T–6	1.40	D–M–6	1.20	D–B–6	1.20

drop gives 0.05 in. permanent set, and on the 100-lb. new section a 12-in. drop gives 0.06-in. set.

We now have established the fact of how small an amount of work in foot-pounds, applied as impact, will produce this permanent set, and let us now try and see what blow will show the brittleness, the main point which, generally speaking, seems to be covered at this time by the *Drop-Test Machine*.

From four heats of Penna. R. R. new section 100-lb. rail a test piece was taken from top, middle and bottom of each ingot.

The heats were marked A, B, C and D. There were six ingots to each heat.

A–T–1	would be "A" heat,	top	rail	from	No. 1	ingot.
A–T–6	" " "A" "	"	"	"	No. 6	"
A–M–1	" " "A" "	mid.	"	"	No. 1	"
A–B–1	" " "A" "	bot.	"	"	No. 1	"

The chemical analysis was as follows:

	C.	Mn.	Phos.	S.	Si.
"A"	.527	1.13	.078	.057	.056
"B"	.488	.93	.076	.066	.063
"C"	.533	1.13	.076	.056	.051
"D"	.495	.93	.073	.056	.064

The results are given in Table III.

It is interesting to note the number of breakages for other than top test bars. The chemicals analyses of four heats were close enough for a comparison.

A set of progressive tests, to show effect of different heights of drop are given in Tables IV and V, and need no explanation.

Now, it may be interesting to note a few typical analyses of broken drop-test pieces, both those which broke under impact and those which did not. These analyses (Tables VI and VII) seem to show that segregation accounted for the failures.

TABLE IV.—TYPE "C" DROP-TEST MACHINE.

Ten 33-ft. individual rails, each cut into six pieces. 2-000-lb. ball. 3-ft. span. 100-lb. new section.

Rail No.	Cut of Ingot.	14-ft. Drop Set. Ins.	15-ft. Drop Set. Ins.	16-ft. Drop Set. Ins.	17-ft. Drop Set. Ins.	18-ft. Drop Set. Ins.	19-ft. Drop Set. Ins.
1	"A"	1.35	1.50	1.60	1.70	1.80	1.85
2	"	1.70	1.65	1.70	1.75	1.90	1.90
3	"	1.30	1.90	1.70	Broke	1.70	1.75
4	"	1.60	1.65	1.75	1.80	1.90	1.95
5	"	1.20	1.30	1.40	1.45	1.60	1.75
6	"C"	1.60	1.60	1.70	1.80	1.95	1.95
7	"	1.25	1.35	1.45	1.55	1.65	1.70
8	"	1.40	1.50	1.60	1.65	1.80	1.90
9	"	1.30	1.40	1.60	1.60	1.70	1.75
10	"	1.70	1.75	1.95	1.90	2.10	2.20

TABLE V.—TYPE "C" DROP-TEST MACHINE.

Individual rails tested at 15-ft. and 19-ft. drop to try to determine whether 15 ft. will show brittleness.

	15-ft. Drop Set. Ins.	19-ft. Drop Set. Ins.
No. 1—1	Broke	1.95
2	Broke	1.95
3	Broke	Broke
No. 2—1	1.65	Broke
2	Broke	2.50
3	Broke	....
No. 3—1	1.30	Broke
2	Broke	1.65
No. 4—1	1.55	1.95
2	1.55	Broke
3	Broke	Broke
No. 5—1	1.50	Broke
2	Broke	1.25
3	Broke	....
No. 6—1	Broke	2.10
2	Broke	Broke
3	1.70	....

TABLE VI.—ANALYSES OF BROKEN TEST PIECES.

	Per cent. C.	Per cent. Mn.	Per cent. Phos.	Per cent. S.	Per cent. Si.
"A"—Top of ingot.					
Top of head	0.451	0.99	0.049	0.041	0.080
Center of head	0.574	1.04	0.083	0.058	0.079
Web	0.681	1.12	0.103	0.065	0.077
Flange	0.454	0.97	0.054	0.028	0.082
"B"—Top of ingot.					
Top of head	0.329	0.91	0.050	0.036	0.065
Center of head	0.488	0.95	0.077	0.060	0.068
Web	0.634	1.06	0.082	0.088	0.061
Flange	0.390	0.89	0.042	0.033	0.067
"C"—Top of ingot.					
Top of head	0.342	1.01	0.042	0.024	0.050
Center of head	0.501	1.07	0.091	0.046	0.054
Web	0.598	1.08	0.111	0.052	0.047
Flange	0.413	0.98	0.049	0.030	0.056

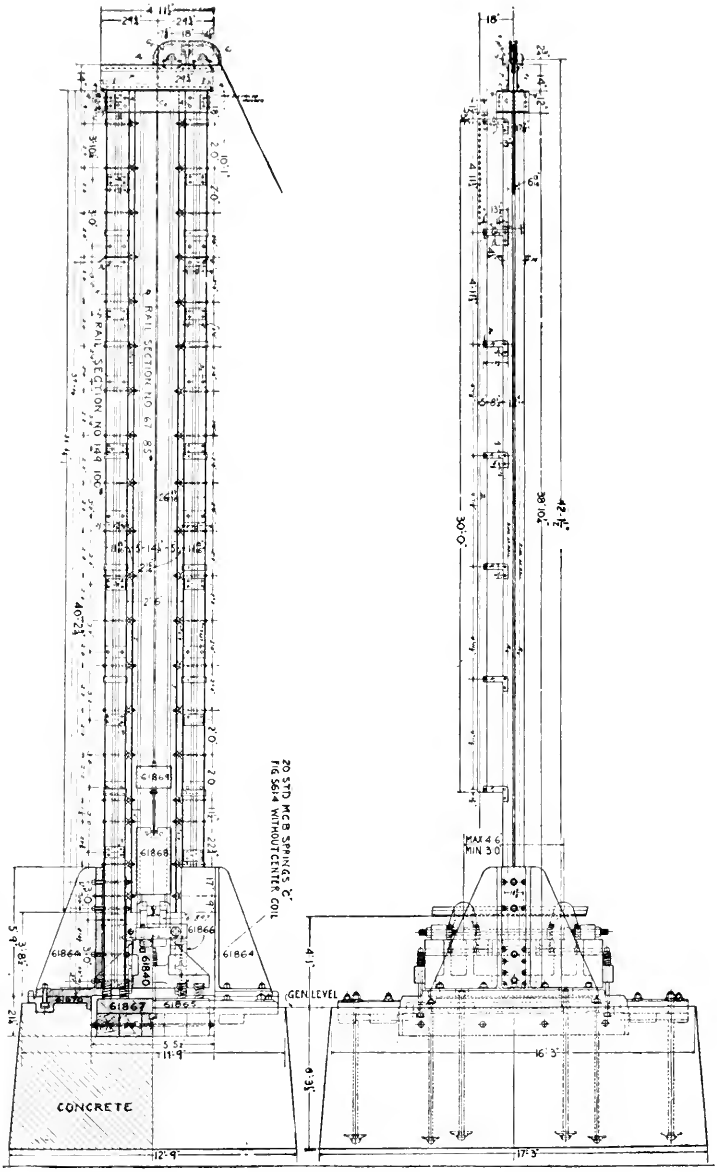


Fig. 3.—Proposed Standard Drop-Testing Machine.

TABLE VII.—ANALYSES OF DROP TESTS WHICH DID NOT FAIL, AND WHICH SHOW SEGREGATION.

	C. Per cent.	Mn. Per cent.	Phos. Per cent.	S. Per cent.	Si. Per cent.
"D"—Top of ingot.					
Top of head....	0.479	0.94	0.070	0.046	0.063
Center of head..	0.482	0.97	0.070	0.049	0.061
Web...........	0.627	1.00	0.084	0.049	0.059
Flange.........	0.482	1.02	0.068	0.050	0.065
"E"—Top of ingot.					
Top of head....	0.462	0.90	0.055	0.050	0.052
Center of head..	0.600	0.98	0.092	0.090	0.056
Web...........	0.661	0.99	0.121	0.108	0.057
Flange.........	0.427	0.90	0.050	0.049	0.051
"F"—Top of ingot.					
Top of head....	0.405	0.97	0.058	0.063	0.049
Center of head..	0.589	1.07	0.095	0.072	0.050
Web...........	0.625	1.04	0.112	0.087	0.051
Flange.........	0.474	0.96	0.061	0.052	0.048

DESCRIPTION OF THE PROPOSED STANDARD DROP-TEST MACHINE.

(SEE FIGS. 3 AND 4.)

(*a*) Anvil to be a solid iron casting, at least 15 ins. thick, weighing, with attachments, 20,000 lbs. At the center of its length, on the top face, a cavity is formed by ribs rising from the body of the anvil, said cavity to provide for the retention of a block of wood. Top face of anvil to contain two dove-tail grooves with finished surfaces suitable for the firm attachment of pedestals holding dies. Maximum distance between centers of dies to be 4 ft. 6 ins., while the standard center distance is to be 3 ft. Anvil to be guided in its vertical movement on removable finished wearing strips. These wearing strips to be suitably attached to the finished edges on the cast-iron supports of column. Bottom surface of anvil block to be generally plane, with local depressions consisting of cored holes $5\frac{3}{4}$ ins. diameter and 3 ins. deep, serving as bearings for the tops of the several springs supports.

(*b*) Anvil to be supported on a set of twenty springs known as the Standard "C" Spring, without center coil, as employed by the Master Car Builders' Association (their Figure 5614). This spring has a free length of $8\frac{1}{4}$ ins., an outside diameter of $5\frac{7}{16}$ ins., and is made from a bar having a diameter of $1\frac{3}{16}$ ins. These springs are to be arranged in groups of five at each corner of the anvil and are to be held in place by means of hubs raised on the top of the base plate, together with the constraint due to the corded holes in the bottom face of the anvil.

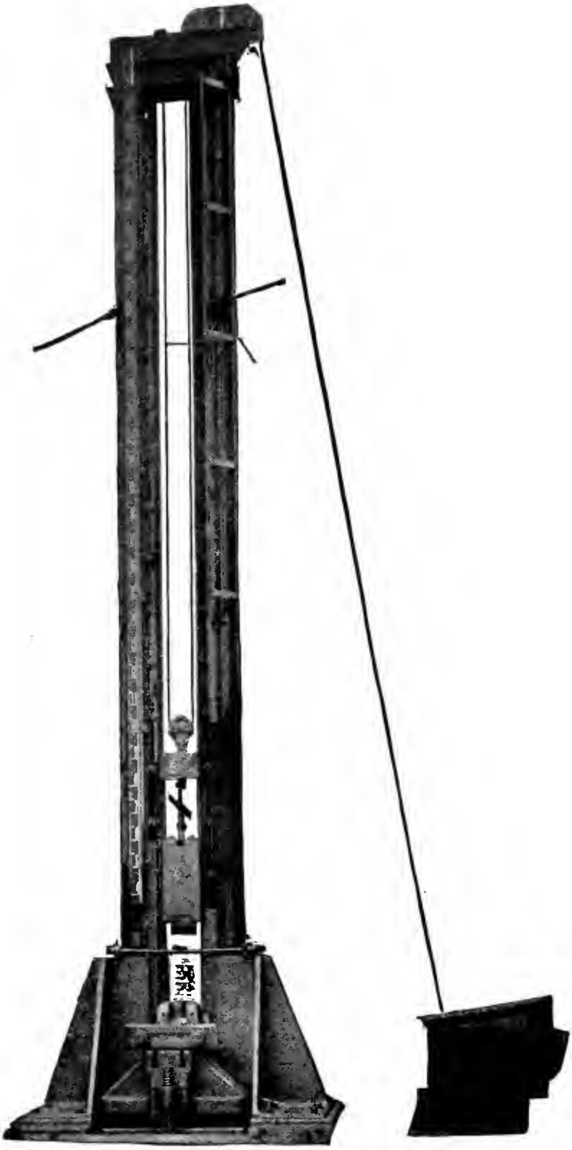


FIG. 4.—Proposed Standard Drop-Testing Machine.

(*c*) The ram is to be made of cast iron, with dove-tail groove at lower end suitable for attachment of a steel striking die. Dimensions of ram to be over all. Length, $35\frac{1}{2}$ ins.; width, 18 ins.; thickness, $11\frac{3}{4}$ ins. Guiding surfaces of ram to consist of two finished grooves, $2\frac{5}{8}$ ins. wide; and $2\frac{1}{16}$ ins. deep, set longitudinally in the center of the narrow faces of the ram. The striking die is to be made of steel, having a radius of striking face of 5 ins. and a length of said face of 12 ins. The tripping head for the ram is to be made of steel and set in center of the top face of the ram. The total weight of ram, striking die, and tripping head combined, is to be 2,000 lbs.

(*d*) Anvil dies to be removable pieces of steel having cylindrical bearing surface for rail of a radius of 5 ins. Said dies to be held in top of adjustable pedestals by means of grooves of rectangular section.

(*e*) Pedestals holding dies to be attached to top face of anvil by means of dove-tail grooves and keys. Said grooves are to be $4\frac{1}{2}$ ins. deep. Keys are to be 1 in. thick, having a bearing over the full length of the base of the pedestal. Pedestals are to be additionally confined by two tie bolts with long threads and set nuts at each of their ends, so that center distance of dies may be adjusted as required to anything between 3 ft. and 4 ft.

TABLE VIII.—PROGRESSIVE DROP TESTS MADE ON STANDARD DROP TEST MACHINE.

Separate pieces used for each drop. All test pieces contained 0.50 per cent. carbon, and 0.91 to 1.00 per cent. manganese. 2,000-lb. ball. 3-ft. span. Temperature, 78° F. May 29, 1908.

	P. R. R. 85-lb. No. 71819-6.		P. R. R. 85-lb. New Section. No. 73685-5.		P. R. R. 100-lb. No. 58129-5.		P. R. R. 100-lb. New Section. No. 73979-4.	
Height of Drop.	Permanent Set.	Recovered Deflection.	Permanent Set.	Recovered Deflection.	Permanent Set.	Recovered Deflection.	Permanent Set.	Recovered Deflection.
	Ins.	Ins.	Ins.	Ins.	Ins.	Ins.	Ins.	Ins.
6 ins.	0.02	0.09	0.02	0.14	0.02	0.13	0.02	0.09
12 ins.	0.08	0.14	0.08	0.13	0.08	0.13	0.06	0.13
18 ins.	0.16	0.16	0.17	0.14	0.14	0.13	0.13	0.14
24 ins.	0.25	0.19	0.22	0.16	0.19	0.14	0.19	0.13
3 ft.	0.38	0.19	0.37	0.16	0.28	0.16	0.25	0.14
4 ft.	0.49	0.22	0.47	0.16	0.40	0.17	0.38	0.14
	Heat No. 71819-6.		Heat No. 73713-5.		Heat No. 50774-5.		Heat No. 73979-4.	
5 ft.	0.60	0.22	0.53	0.19	0.50	0.19	0.45	0.16
6 ft.	0.76	0.22	0.66	0.22	0.64	0.16	0.62	0.22
7 ft.	0.88	0.25	0.76	0.25	0.74	0.17	0.70	0.22
8 ft.	0.99	0.25	0.90	0.25	0.86	0.17	0.81	0.22
9 ft.	1.12	0.27	1.01	0.25	0.98	0.19	0.90	0.22
10 ft.	1.20	0.28	1.13	0.28	1.06	0.19	1.02	0.25

6 ins. inclusive. Tie bolts are to pass freely between the columns supporting guide rails and should be forged to a rectangular section in the vicinity occupied by the ram.

(*f*) Base plate supporting springs and anvil to be of cast iron, rectangular in plan and 8 ins. thick throughout the region covered by the anvil. The upper surface of the base plate will be generally plane, except for local projections in the form of hubs for retaining the springs in position. Base plate must be confined to substructure consisting of foundation and timber cap by four bolts each 2 ins. diameter.

(*g*) A timber foundation cap consisting of a floor made from 12 x 12 in. oak timber bolted together will be the immediate support of the base plate. These timbers are to be 18 ins. longer than the base plate. Local preferences for type of foundation below these timber may be followed, but a substantial bolt attachment of timber floor to substructure is required.

(*h*) The guiding surfaces for the ram are to be the heads of two standard 85-lb. A Section rails. These rails are to provide for a free fall

TABLE IX.—DROP TESTS MADE ON STANDARD DROP-TEST MACHINE.
Separate pieces used for each height of drop. 2,000-lb. ball. 3-ft. span.
Temperature, 82° F. May 20, 1908.

Heat No.	14-ft. Drop. Permanent Set. Ins.	15-ft. Drop. Permanent Set. Ins.	16-ft. Drop. Permanent Set. Ins.	17-ft. Drop. Permanent Set. Ins.	18-ft. Drop. Permanent Set. Ins.	19-ft. Drop. Permanent Set. Ins.
		P. R. R. 100-lb. New Section.				
73931–1	1.25	1.35	1.45	1.60	1.75	1.80
73927–1	1.30	1.35	1.45	1.55	1.65	1.75
73085–1	1.40	1.50	1.55	1.60	1.70	1.75
73990–1	1.45	1.50	1.55	1.65	1.70	1.80
73911–1	1.30	1.45	1.50	1.55	1.65	1.75
73992–3	1.30	1.35	1.50	1.60	1.75	1.80
73978–3	1.45	1.50	1.55	1.70	1.80	1.90
73988–3	1.25	1.40	1.50	1.55	1.65	1.75
73981–3	1.20	Broke	1.40	1.50	1.55	1.70
73546–3	1.60	1.70	1.80	1.85	2.05	2.10
		P. R.R. 100-lb. Section.				
50775–1	Broke	1.30	Broke	1.45	1.50	1.55
46770–1	1.05	1.10	1.20	Broke	1.25	Broke
46404–1	Broke	Broke	Broke	Broke	Broke	1.60
46822–1	1.05	1.10	Broke	1.20	1.30	1.55
46469–4	1.25	1.30	1.40	1.50	1.55	1.70
46723–3	1.35	1.40	1.55	1.65	1.75	1.90
46744–3	1.35	1.45	1.60	1.70	1.80	1.90
40302–3	1.35	1.45	1.55	1.65	1.75	1.85
40304–3	1.30	1.40	1.50	1.55	1.65	1.70
46773–3	1.20	1.30	1.35	1.45	1.55	1.65

TABLE X.—TESTS OF TEN A. S. C. E. 100-LB. RAILS, FROM C CUT OF INGOT, CUT INTO SIX PIECES EACH.

2,000-lb. ball. 3-ft. supports.

	Type "C Machine (20,000-lb. anvil).			Standard Machine.		
Rail No.	Piece No.	15-ft Drop. Permanent Set, ins.	19-ft. Drop. Permanent Set, ins.	Piece No.	15-ft. Drop. Permanent Set, ins.	19-ft. Drop. Permanent Set, ins.
1	1	1.10	....	2	1.10	....
	3	1.10	....	4	1.15	....
	5	1.10	....	6	1.15	....
2	1	1.20	....	2	1.20	....
	3	1.20	....	4	1.20	....
	5	1.20	....	6	1.25	....
3	1	1.20	....	2	1.20	....
	3	1.20	....	4	1.25	....
	5	1.20	....	6	1.25	....
4	1	1.10	....	2	1.10	....
	3	1.15	....	4	1.15	....
	5	Broke	....	6	1.15	....
5	1	1.20	....	2	1.25	....
	3	1.20	....	4	1.25	....
	5	1.20	....	6	1.25	....
6	1	....	1.50	2	....	1.50
	3	....	1.55	4	....	1.55
	5	....	1.50	6	....	1.55
7	1	....	1.60	2	....	1.60
	3	....	1.60	4	....	1.60
	5	....	1.60	6	....	1.60
8	1	....	1.70	2	....	1.70
	3	....	1.70	4	....	1.70
	5	....	1.75	6	....	1.75
9	1	....	1.55	2	....	1.60
	3	....	1.60	4	....	1.60
	5	....	1.60	6	....	1.60
10	1	....	1.60	2	....	1.60
	3	....	1.60	4	....	1.60
	5	....	1.65	6	....	1.65

	15-ft. Blow.	19-ft. Blow.
Depression of anvil..........................	....	$\frac{3}{4}$ in.
Average deflection, Type "C" Machine ..	1.16 ins.	1.606 ins.
" " Standard " ..	1.19 ins.	1.616 ins.

Temperature when tests were made on Type "C" Machine, 58° F.
" " " " " " Standard " 60° F.

of the ram of at least 25 ft., with such additional length as the clearance for disengaging apparatus will require. To enable the removal of the ram without disturbing the column, one of the rail guides shall contain a 6-ft. piece held to its companion by a standard 85 A angle bar. This joint is made about 9 ft. 6 ins. above the base of the supporting column.

(*i*) The superstructure is to be made preferably of steel structural shapes and fastenings providing for a firm support of the rail guides.

The need of a standard machine is apparent when we consider the differences between machines in use, as indicated in the descriptions of Types "A," "B" and "C."

The machine of Type "C" covers most specifications, but is subject to variable sub-soil conditions according to its location. This may be remedied by inserting springs under the anvil, so as to conform to the standard machine. The drop test will then admit of a fair comparison of rails from different mills.

The standard machine, described above, is somewhat heavier in construction throughout than the Standard Axle Machine. Some complaints having been received as to weakness in construction of the Axle Machine, the Standard Drop-Test Machine for rails may also become the standard machine for axles.

Results of tests with Drop-Test Machines are appended, viz:

Table VIII—Progressive tests made with machines of Types "A" and "C."

Table IX—Results for different heights of drop for rails from some position in ingot.

Table X—Results of tests of rails, each cut into six pieces; three pieces tested with Type "C" Machine, three pieces tested with standard machine.

The results in Table IX are interesting in showing that resultant set in the test rails compare favorably with the Type "C" test results.

Fig. 3 shows a machine constructed so as to have all the features of the standard machine. The machine also has an automatic tripping device whereby the fall of tup will be constant when once set for a height. The photograph (Fig 4) gives a general idea of the standard machine on which the above tests were made.

REPORT OF COMMITTEE B ON STANDARD SPECIFICATIONS FOR CAST IRON AND FINISHED CASTINGS.

The chairmen of the sub-committees on Cylinders, on Car Wheels, on General Castings, and on Testing Cast Iron, report that they have no changes to suggest in the specifications that were adopted some years ago by the Society.

The chairman of the sub-committee on Cast Iron Pipe makes a similar report, stating further, however, that the American Water Works Association, who have for the past few years been considering the adoption of standard specifications, and for that purpose have had before them the various prominent cast iron pipe specifications used in the United States, at their annual convention last May adopted a standard specification almost identical in language and form with that adopted by this Society. This action by the American Water Works Association in virtually adopting our specifications opens the way to a final form of standard specification for cast iron pipe in the United States.

The sub-committee on Pig Iron held a session at Columbia University in January, and submit the following report:

Report of Sub-Committee on Pig Iron.

The sub-committee on Standard Specifications for Foundry Pig Iron believe that, as buyers are now so largely basing their contracts upon analysis, and since those who are still adhering to the old nomenclature of grading fully appreciate the analysis which such grading indicates, the time has come for proposing that hereafter pig iron shall always be bought by analysis. But in abandoning grading by fracture for buying by analysis, the specifications issued by an association such as the American Society for Testing Materials should be stated in the broadest possible terms.

It then becomes necessary to determine only the percentages of each element, the variations therein, and the allowed departures

therefrom, together with definite modes of sampling and reaching adjustments of such differences as may occur.

They therefore submit the following specifications for consideration by the Society:

PROPOSED STANDARD SPECIFICATIONS FOR FOUNDRY PIG IRON.

Analysis.—It is recommended that all purchases be made by analysis.

Sampling.—Each carload, or its equivalent, shall be considered as a unit. At least one pig shall be selected from each two tons of every carload, and so as to represent it fairly. Drillings shall be taken so as to represent fairly the fracture surface of each pig. The sample analyzed shall consist of an equal quantity of drillings from each pig, well mixed and ground before analysis.

Percentage of Elements.—Opposite each percentage of the different elements a syllable has been affixed so that buyers, by combining these syllables can form a code word to be used in telegraphing such inquiries as they may desire to make.

Silicon.		*Sulphur.*		*Phosphorus.*		*Manganese.*	
Per cent.	Symbol.	Per cent.	Symbol.	Per cent.	Symbol.	Per cent.	Symbol.
0.50	Ca	0.04	Sa	0.25	Pa	0.50	Ma
1.00	Ce	0.05	Se	0.50	Pe	0.75	Me
1.50	Ci	0.06	Si	0.75	Pi	1.00	Mi
2.00	Co	0.07	So	1.00	Po	1.25	Mo
2.50	Cu	0.08	Su	1.25	Pu	1.50	Mu
3.00	Cy	0.09	Sy	1.50	Py	1.75	My
0.25 allowable variation.		maxima, gravimetric method.		0.125 allowable variation.		0.125 allowable variation.	

	Sil.	Sul.	Phos.	Mang.
Example.—Code word Ci-se-pi-ma denotes	1.50	0.05	0.75	0.50

These specifications were laid before the meeting of the Foundrymen's Association, held this month at Toronto, by Dr. Moldenke, the Secretary of this Committee.

They received earnest consideration by that body, which passed the following resolutions:

Resolved, That the American Foundrymen's Association in Convention deprecate the use of grade numbers of any kind in specifications for foundry pig iron, and go on record as favoring specifications by chemical analysis only.

Resolved, That the American Foundrymen's Association requests the American Society for Testing Materials to coöperate with it and other Foundrymen's Associations, in bringing about specifications for foundry pig iron along the lines of the preceding resolution.

Following the above resolutions Dr. Moldenke, as acting for the Foundrymen's Association, suggests that the American Society for Testing Materials meet the Foundrymen as requested, and that the Society at this meeting pass the following resolutions:*

WHEREAS, The American Foundrymen's Association has asked for coöperation in bringing about specifications for pig iron on the basis of chemical analysis:

Resolved, That Committee B be authorized to join with the American Foundrymen's Association and similar associations in the preparation of specifications for foundry pig iron.

Your Committee would therefore request, that in accepting their report, such resolutions be adopted, since action on these lines will bring together those handling pig iron in such a way that specifications will be agreed upon which will permit of universal adoption by both buyer and seller.

The sub-committees on the Influence of the Addition of Special Metals to Cast Iron, and on Micro-Structure of Cast Iron have no reports to present.

Respectfully submitted on behalf of the Committee,

WALTER WOOD,
Chairman.

RICHARD MOLDENKE,
Secretary.

* These resolutions were adopted.

REPORT OF COMMITTEE C ON STANDARD SPECIFICATIONS FOR CEMENT.

The Committee on Standard Specifications for Cement has had under consideration the various changes suggested by the members of the Society and others.

Several hundred letters were sent to those possessing testing laboratories for cement, including engineers, architects, contractors and others, with the request that they transmit to the Secretary of the Committee any criticism of the methods proposed by the special committee of the American Society of Civil Engineers; and, also, state what specifications they were using, and if they were using the Standard Specifications of the American Society for Testing Materials, whether they had any criticisms to offer in regard to those specifications. To these letters 143 replies were received, covering every important laboratory in the country and embracing almost all of the important works of construction. Of the total number, 93 stated that they were using the specifications of the American Society for Testing Materials (of this number 72 reported that the specifications were satisfactory in every respect); 12 reported that they were using the specifications recommended by a committee of the Corps of Engineers, U. S. Army (all of these were army officers in charge of government work); 30 had their own specifications (these differed very slightly from those of the American Society for Testing Materials); and 8 reported that they were not purchasers of cement, and had no specifications. The various recommendations which were made concerning the standard specifications were collated by the Secretary and sent to the members of the Committee.

At a meeting held at the Engineers' Club of Philadelphia, April 13, 1908, the following amendments were adopted which were subsequently ratified by the members of the Committee by letter-ballot, and are herewith submitted for adoption by the Society:

General Observations.

First sentence, Paragraph 3, "Specific gravity is useful in detecting adulteration or under-burning." Amend by striking out the words "or under-burning."

Amend Paragraph 9 by adding at the end of the second sentence the words "using a new sample." The sentence thus amended to read "The cement may, however, be held for twenty-eight days, and a retest made at the end of that period, using a new sample."

Insert at the end of Paragraph 9 as principal heading the word "Specifications."

General Conditions.

Amend Paragraph 9 to read as follows: "All tests shall be made in accordance with the methods proposed by the Committee on Uniform Tests of Cement of the American Society of Civil Engineers, presented to the Society January 21, 1903, amended January 20, 1904, and January 15, 1908, with all subsequent amendments thereto."

Strike out Paragraph 12 covering the requirements for specific gravity of natural cement.

Amend Paragraph 14 to read as follows: "It shall not develop initial set in less than 10 minutes; and shall not develop hard set in less than 30 minutes or in more than 3 hours.

Insert at the end of the requirements of Paragraph 15, as a note, the following: "If the minimum strength is not specified the mean of the above values shall be taken as the minimum strength required."

Amend Paragraph 19 to read, "The specific gravity of the cement, ignited at a low red heat, shall not be less than 3.10 and the cement shall not show a loss on ignition of more than 4 per cent."

Amend Paragraph 21 to read, "It shall not develop initial set in less than 30 minutes; and must develop hard set in not less than one hour, nor more than ten hours."

Insert after the requirements in Paragraph 22 the following note: "If the minimum strength is not specified the

mean of the above values shall be taken as the minimum strength required."

Renumber all paragraphs.

Respectfully submitted on behalf of the Committee,

GEORGE F. SWAIN,
Chairman.

RICHARD L. HUMPHREY,
Secretary.

NOTE.—The above proposed amendments to the Standard Specifications for Cement were adopted by letter-ballot on August 15, 1908. The specifications as amended, as well as the methods of testing proposed by the Committee on Uniform Tests of Cement of the American Society of Civil Engineers, in their amended form, follow this report.

AMERICAN SOCIETY FOR TESTING MATERIALS

PHILADELPHIA, PA., U. S. A.

AFFILIATED WITH THE

INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

STANDARD SPECIFICATIONS FOR CEMENT.

ADOPTED AUGUST 15, 1908.

GENERAL OBSERVATIONS.

1. These remarks have been prepared with a view of pointing out the pertinent features of the various requirements and the precautions to be observed in the interpretation of the results of the tests.

2. The Committee would suggest that the acceptance or rejection under these specifications be based on tests made by an experienced person having the proper means for making the tests.

SPECIFIC GRAVITY.

3. Specific gravity is useful in detecting adulteration. The results of tests of specific gravity are not necessarily conclusive as an indication of the quality of a cement, but when in combination with the results of other tests may afford valuable indications.

FINENESS.

4. The sieves should be kept thoroughly dry.

TIME OF SETTING.

5. Great care should be exercised to maintain the test pieces under as uniform conditions as possible. A sudden change or wide range of temperature in the room in which the tests are made, a very dry or humid atmosphere, and other irregularities vitally affect the rate of setting.

TENSILE STRENGTH.

6. Each consumer must fix the minimum requirements for tensile strength to suit his own conditions. They shall, however, be within the limits stated.

CONSTANCY OF VOLUME.

7. The tests for constancy of volume are divided into two classes, the first normal, the second accelerated. The latter should be regarded as a precautionary test only, and not infallible. So many conditions enter into the making and interpreting of it that it should be used with extreme care.

8. In making the pats the greatest care should be exercised to avoid initial strains due to molding or to too rapid drying-out during the first twenty-four hours. The pats should be preserved under the most uniform conditions possible, and rapid changes of temperature should be avoided.

9. The failure to meet the requirements of the accelerated tests need not be sufficient cause for rejection. The cement may, however, be held for twenty-eight days, and a retest made at the end of that period, using a new sample. Failure to meet the requirements at this time should be considered sufficient cause for rejection, although in the present state of our knowledge it cannot be said that such failure necessarily indicates unsoundness, nor can the cement be considered entirely satisfactory simply because it passes the tests.

SPECIFICATIONS.

GENERAL CONDITIONS.

1. All cement shall be inspected.

2. Cement may be inspected either at the place of manufacture or on the work.

3. In order to allow ample time for inspecting and testing, the cement should be stored in a suitable weather-tight building having the floor properly blocked or raised from the ground.

4. The cement shall be stored in such a manner as to permit easy access for proper inspection and identification of each shipment.

5. Every facility shall be provided by the Contractor and a period of at least twelve days allowed for the inspection and necessary tests.

6. Cement shall be delivered in suitable packages with the brand and name of manufacturer plainly marked thereon.

7. A bag of cement shall contain 94 pounds of cement net. Each barrel of Portland cement shall contain 4 bags, and each barrel of natural cement shall contain 3 bags of the above net weight.

8. Cement failing to meet the seven-day requirements may be held awaiting the results of the twenty-eight day tests before rejection.

9. All tests shall be made in accordance with the methods proposed by the Committee on Uniform Tests of Cement of the American Society of Civil Engineers, presented to the Society January 21, 1903, and amended January 20, 1904, and January 15, 1908, with all subsequent amendments thereto. (See addendum to these specifications.)

10. The acceptance or rejection shall be based on the following requirements:

NATURAL CEMENT.

11. *Definition.* This term shall be applied to the finely pulverized product resulting from the calcination of an argillaceous limestone at a temperature only sufficient to drive off the carbonic acid gas.

FINENESS.

12. It shall leave by weight a residue of not more than 10 per cent. on the No. 100, and 30 per cent. on the No. 200 sieve.

TIME OF SETTING.

13. It shall not develop initial set in less than ten minutes; and shall not develop hard set in less than thirty minutes, or in more than three hours.

TENSILE STRENGTH.

14. The minimum requirements for tensile strength for briquettes one inch square in cross section shall be within the fol-

lowing limits, and shall show no retrogression in strength within the periods specified:*

Age.	*Neat Cement.*	*Strength.*
24 hours in moist air		50–100 lbs.
7 days (1 day in moist air, 6 days in water)		100–200 "
28 days (1 " " " 27 " ")		200–300 "

One Part Cement, Three Parts Standard Sand.

7 days (1 day in moist air, 6 days in water)	25–75 "
28 days (1 " " " 27 " ")	75–150 "

If the minimum strength is not specified, the mean of the above values shall be taken as the minimum strength required.

CONSTANCY OF VOLUME.

15. Pats of neat cement about three inches in diameter, one-half inch thick at center, tapering to a thin edge, shall be kept in moist air for a period of twenty-four hours.

(*a*) A pat is then kept in air at normal temperature.

(*b*) Another is kept in water maintained as near 70° F. as practicable.

16. These pats are observed at intervals for at least 28 days, and, to satisfactorily pass the tests, should remain firm and hard and show no signs of distortion, checking, cracking or disintegrating.

PORTLAND CEMENT.

17. *Definition.* This term is applied to the finely pulverized product resulting from the calcination to incipient fusion of an intimate mixture of properly proportioned argillaceous and calcareous materials, and to which no addition greater than 3 per cent. has been made subsequent to calcination.

SPECIFIC GRAVITY.

18. The specific gravity of the cement, ignited at a low red heat, shall be not less than 3.10, and the cement shall not show a loss on ignition of more than 4 per cent.

* For example the minimum requirement for the twenty-four hour neat cement test should be some specified value within the limits of 50 and 100 pounds, and so on for each period stated.

FINENESS.

19. It shall leave by weight a residue of not more than 8 per cent. on the No. 100, and not more than 25 per cent. on the No. 200 sieve.

TIME OF SETTING.

20. It shall not develop initial set in less than thirty minutes; and must develop hard set in not less than one hour, nor more than ten hours.

TENSILE STRENGTH.

21. The minimum requirements for tensile strength for briquettes one inch square in section shall be within the following limits, and shall show no retrogression in strength within the periods specified:*

Age.	*Neat Cement.*	*Strength.*
24 hours in moist air		150–200 lbs.
7 days (1 day in moist air, 6 days in water)		450–550 "
28 days (1 " " " 27 " ")		550–650 "

One Part Cement, Three Parts Standard Sand.

7 days (1 day in moist air, 6 days in water)	150–200 lbs.
28 days (1 " " " 27 " ")	200–300 "

If the minimum strength is not specified, the mean of the above values shall be taken as the minimum strength required.

CONSTANCY OF VOLUME.

22. Pats of neat cement about three inches in diameter, one-half inch thick at the center, and tapering to a thin edge, shall be kept in moist air for a period of twenty-four hours.

(*a*) A pat is then kept in air at normal temperature and observed at intervals for at least 28 days.

(*b*) Another pat is kept in water maintained as near 70° F. as practicable, and observed at intervals for at least 28 days.

* For example the minimum requirement for the twenty-four hour neat cement test should be some specified value within the limits of 150 and 200 pounds, and so on for each period stated.

(*c*) A third pat is exposed in any convenient way in an atmosphere of steam, above boiling water, in a loosely closed vessel for five hours.

23. These pats, to satisfactorily pass the requirements, shall remain firm and hard and show no signs of distortion, checking, cracking, or disintegrating.

SULPHURIC ACID AND MAGNESIA.

24. The cement shall not contain more than 1.75 per cent. of anhydrous sulphuric acid (SO_3), nor more than 4 per cent. of magnesia (MgO).

ADDENDUM.

ABSTRACT OF METHODS RECOMMENDED BY THE SPECIAL COMMITTEE ON UNIFORM TESTS OF CEMENT OF THE AMERICAN SOCIETY OF CIVIL ENGINEERS.

SAMPLING.

1.—*Selection of Sample.*—The sample shall be a fair average of the contents of the package; it is recommended that, where conditions permit, one barrel in every ten be sampled.

2.—Samples should be passed through a sieve having twenty meshes per linear inch, in order to break up lumps and remove foreign material; this is also a very effective method for mixing them together in order to obtain an average. For determining the characteristics of a shipment of cement, the individual samples may be mixed and the average tested; where time will permit, however, it is recommended that they be tested separately.

3.—*Method of Sampling.*—Cement in barrels should be sampled through a hole made in the center of one of the staves, midway between the heads, or in the head, by means of an auger or a sampling iron similar to that used by sugar inspectors. If in bags, it should be taken from surface to center.

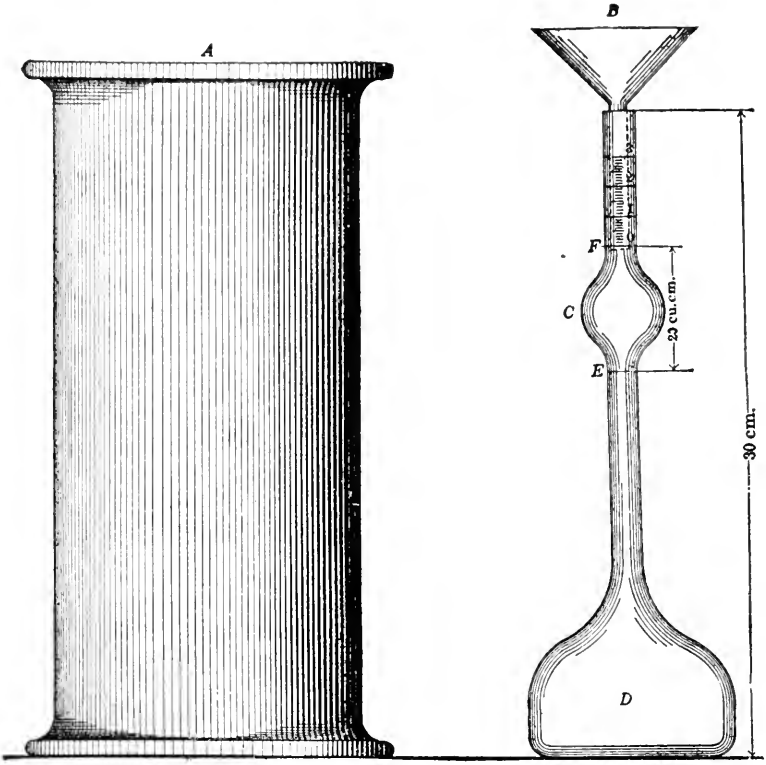
CHEMICAL ANALYSIS.

4.—*Method* —As a method to be followed for the analysis of cement, that proposed by the Committee on Uniformity in the Analysis of Materials for the Portland Cement Industry, of the New York Section of the Society for Chemical Industry, and published in the *Journal* of the Society for January 15, 1902, is recommended.

SPECIFIC GRAVITY.

5.—*Apparatus and Method.*—The determination of specific gravity is most conveniently made with Le Chatelier's apparatus. This consists of a flask (*D*), Fig. 1, of 120 cu. cm. (7.32 cu. ins.) capacity, the neck of which is about 20 cm. (7.87 ins.) long; in the middle of this neck is a bulb (*C*), above and below which are two marks (*F*) and (*E*); the volume between these marks is 20 cu. cm. (1.22 cu. ins.). The neck has a diameter of about 9 mm. (0.35 in.), and is graduated into tenths of cubic centimeters above the mark (*F*).

6.—Benzine (62° Baumé naphtha), or kerosene free from water should be used in making the determination.



LE CHATELIER'S SPECIFIC GRAVITY APPARATUS
FIG. 1.

7.—The specific gravity can be determined in two ways.

(1) The flask is filled with either of these liquids to the lower mark (*E*), and 64 gr. (2.25 oz.) of powder, cooled to the temperature of the liquid, is gradually introduced through the funnel (*B*) [the stem of which extends into the flask to the top of the bulb (*C*)], until the upper mark (*F*) is reached. The difference in weight between the cement remaining and the original quantity (64 gr.) is the weight which has displaced 20 cu. cm.

8.—(2) The whole quantity of the powder is introduced, and the level of the liquid rises to some division of the graduated neck. This reading plus 20 cu. cm. is the volume displaced by 64 gr. of the powder.

9.—The specific gravity is then obtained from the formula:

$$\text{Specific Gravity} = \frac{\text{Weight of Cement, in grams.}}{\text{Displaced Volume, in cubic centimeters.}}$$

10.—The flask, during the operation, is kept immersed in water in a jar (*A*), in order to avoid variations in the temperature of the liquid. The results should agree within 0.01. The determination of specific gravity should be made on the cement as received; and, should it fall below 3.10, a second determination should be made on the sample ignited at a low red heat.

11.—A convenient method for cleaning the apparatus is as follows: The flask is inverted over a large vessel, preferably a glass jar, and shaken vertically until the liquid starts to flow freely; it is then held still in a vertical position until empty; the remaining traces of cement can be removed in a similar manner by pouring into the flask a small quantity of clean liquid and repeating the operation.

FINENESS.

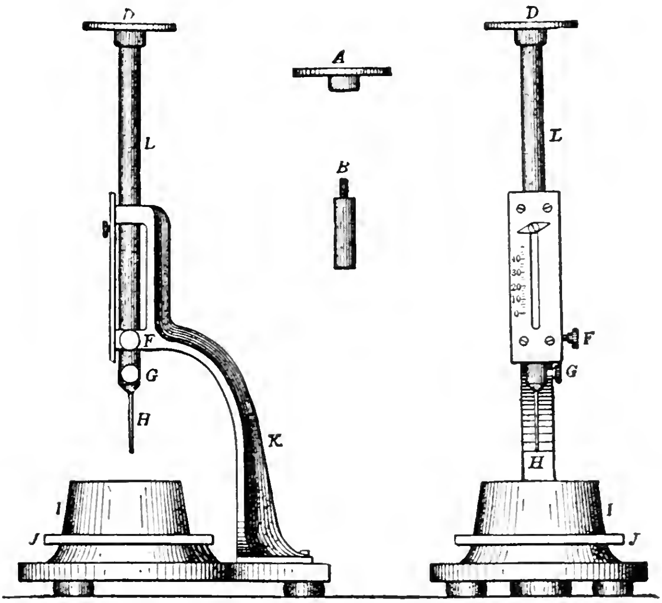
12.—*Apparatus.*—The sieves should be circular, about 20 cm. (7.87 ins.) in diameter, 6 cm. (2.36 ins.) high, and provided with a pan, 5 cm. (1.97 ins.) deep, and a cover.

13.—The wire cloth should be of brass wire having the following diameters:

No. 100, 0.0045 in.; No. 200, 0.0024 in.

14.—This cloth should be mounted on the frames without distortion; the mesh should be regular in spacing and be within the following limits:

No. 100, 96 to 100 meshes to the linear inch.
No. 200, 188 to 200 " " " "



VICAT NEEDLE.
FIG. 2.

15.—Fifty grams (1.76 oz.) or 100 gr. (3.25 oz.) should be used for the test, and dried at a temperature of 100° C. (212° F.) prior to sieving.

16.—*Method.*—The thoroughly dried and coarsely screened sample is weighed and placed on the No. 200 sieve, which, with pan and cover attached, is held in one hand in a slightly inclined position, and moved forward and backward, at the same time striking the side gently with the palm of the other hand, at the rate of about 200 strokes per minute. The operation is continued until not more than one-tenth of 1 per cent passes through after one minute of continuous sieving. The residue is weighed, then placed on the No. 100 sieve and the operation repeated. The work may be expedited by placing in the sieve a small quantity of steel shot. The results should be reported to the nearest tenth of 1 per cent.

NORMAL CONSISTENCY.

17.—*Method.*—This can best be determined by means of *Vicat Needle Apparatus*, which consists of a frame (*K*), Fig. 2, bearing a movable rod (*L*), with the cap (*A*) at one end, and at the other the cylinder (*B*), 1 cm. (0.39 in.) in diameter, the cap, rod and cylinder weighing 300 gr. (10.58 oz.). The rod, which can be held in any desired position by a screw (*F*), carries an indicator, which moves over a scale (graduated to centimeters) attached to the frame (*K*). The paste is held by a conical, hard-rubber ring (*I*), 7 cm. (2.76 ins.) in diameter at the base, 4 cm. (1.57 ins.) high, resting on a glass plate (*J*), about 10 cm. (3.94 ins.) square.

18.—In making the determination, the same quantity of cement as will be subsequently used for each batch in making the briquettes (but not less than 500 grams) is kneaded into a paste, as described in paragraph 39, and quickly formed into a ball with the hands, completing the operation by tossing it six times from one hand to the other, maintained 6 ins. apart; the ball is then pressed into the rubber ring, through the larger opening, smoothed off and placed (on its large end) on a glass plate and the smaller end smoothed off with a trowel; the paste, confined in the ring, resting on the plate, is placed under the rod bearing the cylinder, which is brought in contact with the surface and quickly released.

19.—The paste is of normal consistency when the cylinder penetrates to a point in the mass 10 mm. (0.39 in.) below the top of the ring. Great care must be taken to fill the ring exactly to the top.

20.—The trial pastes are made with varying percentages of water until the correct consistency is obtained.

NOTE. The Committee on Standard Specifications inserts the following table for temporary use to be replaced by one to be devised by the Committee of the American Society of Civil Engineers.

PERCENTAGE OF WATER FOR STANDARD MIXTURES.

Neat	1–1	1–2	1–3	1–4	1–5	Neat	1–1	1–2	1–3	1–4	1–5
18	12.0	10.0	9.0	8.4	8.0	33	17.0	13.3	11.5	10.4	9.6
19	12.3	10.2	9.2	8.5	8.1	34	17.3	13.6	11.7	10.5	9.7
20	12.7	10.4	9.3	8.7	8.2	35	17.7	13.8	11.8	10.7	9.9
21	13.0	10.7	9.5	8.8	8.3	36	18.0	14.0	12.0	10.8	10.0
22	13.3	10.9	9.7	8.9	8.4	37	18.3	14.2	12.2	10.9	10.1
23	13.7	11.1	9.8	9.1	8.5	38	18.7	14.4	12.3	11.1	10.2
24	14.0	11.3	10.0	9.2	8.6	39	19.0	14.7	12.5	11.2	10.3
25	14.3	11.6	10.2	9.3	8.8	40	19.3	14.9	12.7	11.3	10.4
26	14.7	11.8	10.3	9.5	8.9	41	19.7	15.1	12.8	11.5	10.5
27	15.0	12.0	10.5	9.6	9.0	42	20.0	15.3	13.0	11.6	10.6
28	15.3	12.2	10.7	9.7	9.1	43	20.3	15.6	13.2	11.7	10.7
29	15.7	12.5	10.8	9.9	9.2	44	20.7	15.8	13.3	11.9	10.8
30	16.0	12.7	11.0	10.0	9.3	45	21.0	16.0	13.5	12.0	11.0
31	16.3	12.9	11.2	10.1	9.4	46	21.3	16.1	13.7	12.1	11.1
32	16.7	13.1	11.3	10.3	9.5						

	1 to 1	1 to 2	1 to 3	1 to 4	1 to 5
Cement. .	500	333	250	200	167
Sand . . .	500	666	750	800	833

TIME OF SETTING.

21.—*Method.*—For this purpose the Vicat Needle, which has already been described in paragraph 17, should be used.

22.—In making the test, a paste of normal consistency is molded and placed under the rod (*L*), Fig. 2, as described in paragraph 18; this rod, bearing the cap (*D*) at one end and the needle (*H*), 1 mm. (0.039 in.) in diameter, at the other, weighing 300 gr. (10.58 oz.). The needle is then carefully brought in contact with the surface of the paste and quickly released.

23.—The setting is said to have commenced when the needle ceases to pass a point 5 mm. (0.20 in.) above the upper surface of the glass plate, and is said to have terminated the moment the needle does not sink visibly into the mass.

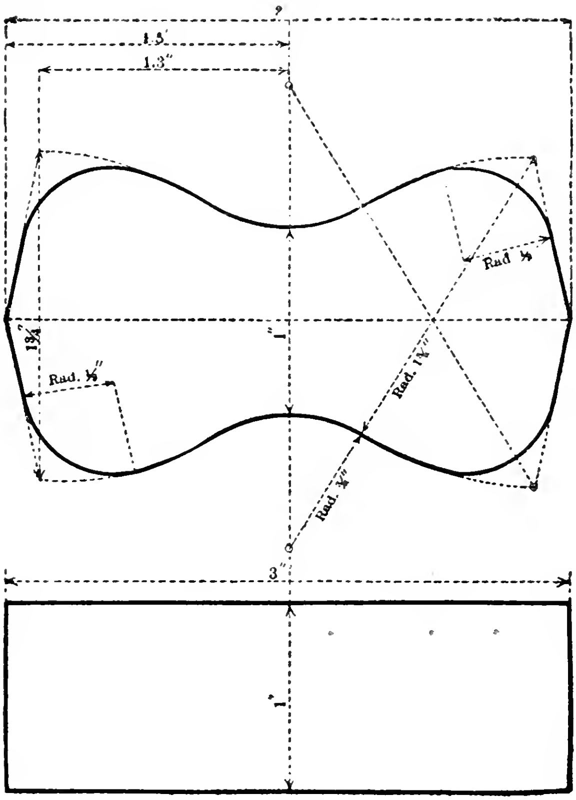
24.—The test pieces should be stored in moist air during the test; this is accomplished by placing them on a rack over water contained in a pan and covered with a damp cloth, the cloth to be kept away from them by means of a wire screen; or they may be stored in a moist box or closet.

25.—Care should be taken to keep the needle clean, as the collection of cement on the sides of the needle retards the penetration, while cement on the point reduces the area and tends to increase the penetration.

26.—The determination of the time of setting is only approximate, being materially affected by the temperature of the mixing water, the temperature and humidity of the air during the test, the percentage of water used, and the amount of molding the paste receives.

STANDARD SAND.

27.—For the present, the Committee recommends the natural sand from Ottawa, Ill., screened to pass a sieve having 20 meshes per linear inch and retained on a sieve having 30 meshes per linear inch; the wires to have diameters of 0.0165 and 0.0112 in., respectively, *i. e.* half the width of the opening in each case. Sand having passed the No. 20 sieve shall be considered standard when not more than 1 per cent passes a No. 30 sieve after one minute continuous sifting of a 500-gram sample.*



DETAILS FOR BRIQUETTE.
FIG. 3.

*The Sandusky Portland Cement Company, of Sandusky, Ohio, has agreed to undertake the preparation of this sand and to furnish it at a price only sufficient to cover the actual cost of preparation.

FORM OF BRIQUETTE.

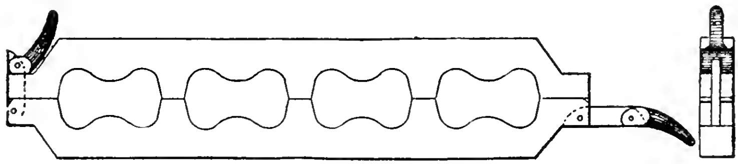
28.—While the form of the briquette recommended by a former Committee of the Society is not wholly satisfactory, this Committee is not prepared to suggest any change, other than rounding off the corners by curves of ½-in. radius, Fig. 3.

MOLDS.

29.—The molds should be made of brass, bronze or some equally non-corrodible material, having sufficient metal in the sides to prevent spreading during molding.

30.—Gang molds, which permit molding a number of briquettes at one time, are preferred by many to single molds; since the greater quantity of mortar that can be mixed tends to produce greater uniformity in the results. The type shown in Fig. 4 is recommended.

31.—The molds should be wiped with an oily cloth before using.



DETAILS FOR GANG MOULD.
FIG. 4.

MIXING.

32.—All proportions should be stated by weight; the quantity of water to be used should be stated as a percentage of the dry material.

33.—The metric system is recommended because of the convenient relation of the gram and the cubic centimeter.

34.—The temperature of the room and the mixing water should be as near 21° C. (70° F.) as it is practicable to maintain it.

35.—The sand and cement should be thoroughly mixed dry. The mixing should be done on some non-absorbing surface, preferably plate glass. If the mixing must be done on an absorbing surface it should be thoroughly dampened prior to use.

36.—The quantity of material to be mixed at one time depends on the number of test pieces to be made; about 1,000 gr. (35.28 oz.) makes a convenient quantity to mix, especially by hand methods.

37.—*Method.*—The material is weighed and placed on the mixing table, and a crater formed in the center, into which the proper percentage of clean water is poured; the material on the outer edge is turned into the crater by the aid of a trowel As soon as the water has been absorbed, which should not require more than one minute, the operation is completed by vigorously kneading with the hands for an additional 1½ minutes, the process being similar to that used in kneading dough. A sand-glass affords a convenient guide for the time of kneading. During the operation of mixing, the hands should be protected by gloves, preferably of rubber.

MOLDING.

38.—Having worked the paste or mortar to the proper consistency, it is at once placed in the molds by hand.

39.—*Method.*—The molds should be filled immediately after the mixing is completed, the material pressed in firmly with the fingers, and smoothed off with a trowel, without mechanical ramming; the material should be heaped up on the upper surface of the mold, and, in smoothing off, the trowel should be drawn over the mold in such a manner as to exert a moderate pressure on the excess material. The mold should be turned over and the operation repeated.

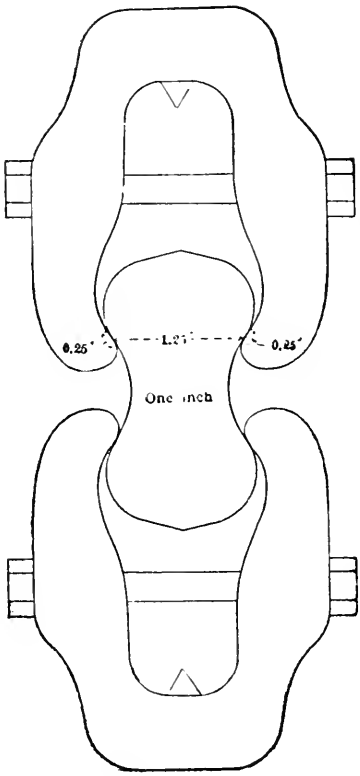
40.—A check upon the uniformity of the mixing and molding is afforded by weighing the briquettes just prior to immersion, or upon removal from the moist closet. Briquettes which vary in weight more than 3 per cent from the average should not be tested.

STORAGE OF THE TEST PIECES.

41.—During the first 24 hours after molding, the test pieces should be kept in moist air to prevent them from drying out.

42.—A moist closet or chamber is so easily devised that the use of the damp cloth should be abandoned if possible. Covering the test pieces with a damp cloth is objectionable, as commonly used, because the cloth may dry out unequally, and in consequence the test pieces are not all maintained under the same condition. Where a moist closet is not available, a cloth may be used and kept uniformly wet by immersing the ends in water. It should be kept from direct contact with the test pieces by means of a wire screen or some similar arrangement.

43.—A moist closet consists of a soapstone or slate box, or a metal-lined wooden box—the metal lining being covered with felt and this felt kept wet. The bottom of the box is so constructed as to hold water, and the sides are provided with cleats for holding glass shelves on which to place the briquettes. Care should be taken to keep the air in the closet uniformly moist.



FORM OF CLIP.
FIG. 5.

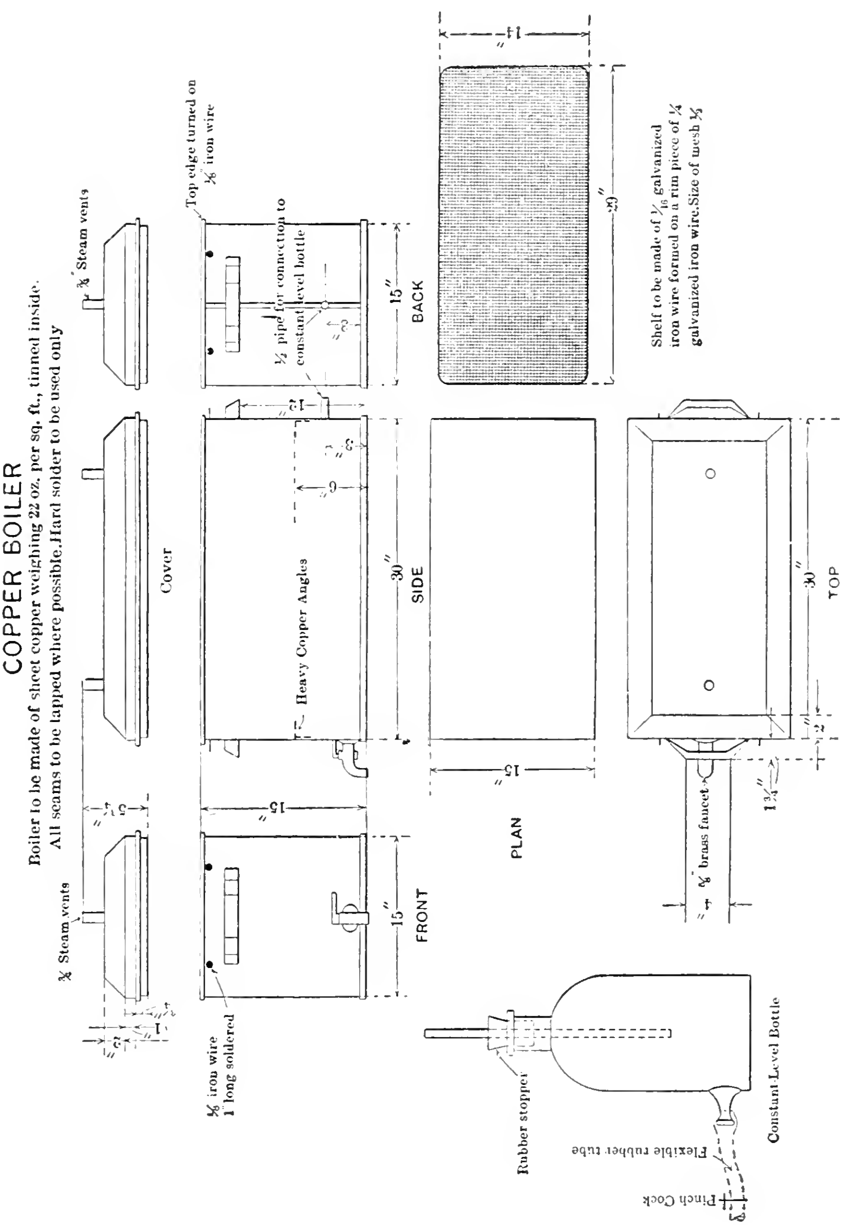

COPPER BOILER
Boiler to be made of sheet copper weighing 22 oz. per sq. ft., tinned inside.
All seams to be lapped where possible. Hard solder to be used only
Cover
Steam vents
Top edge turned on 1/8" iron wire
1/2 pipe for connection to constant level bottle
Heavy Copper Angles
BACK
SIDE
FRONT
PLAN
TOP
Shelf to be made of 1/16 galvanized iron wire formed on a rim piece of 1/4 galvanized iron wire. Size of mesh 1/2
brass faucet
Rubber stopper
Constant Level Bottle
Flexible rubber tube
Pinch Cock
1" long soldered

FIG. 6.

44.—After 24 hours in moist air, the test pieces for longer periods of time should be immersed in water maintained as near 21° C. (70° F.) as practicable; they may be stored in tanks or pans, which should be of non-corrodible material.

TENSILE STRENGTH.

45.—The tests may be made on any standard machine. A solid metal clip, as shown in Fig. 5, is recommended. This clip is to be used without cushioning at the points of contact with the test specimen. The bearing at each point of contact should be ¼ in. wide, and the distance between the center of contact on the same clip should be 1¼ ins.

46.—Test pieces should be broken as soon as they are removed from the water. Care should be observed in centering the briquettes in the testing machine, as cross-strains, produced by improper centering, tend to lower the breaking strength. The load should not be applied too suddenly, as it may produce vibration, the shock from which often breaks the briquette before the ultimate strength is reached. Care must be taken that the clips and the sides of the briquette be clean and free from grains of sand or dirt, which would prevent a good bearing. The load should be applied at the rate of 600 lbs. per minute. The average of the briquettes of each sample tested should be taken as the test, excluding any results which are manifestly faulty.

CONSTANCY OF VOLUME.

47.—*Methods.*—Tests for constancy of volume are divided into two classes: (1) normal tests, or those made in either air or water maintained at about 21° C. (70° F.), and (2) accelerated tests, or those made in air, steam or water at a temperature of 45° C. (115° F) and upward. The test pieces should be allowed to remain 24 hours in moist air before immersion in water or steam, or preservation in air.

48.—For these tests, pats about 7½ cm. (2.95 ins.) in diameter, 1¼ cm. (0.49 in.) thick at the center, and tapering to a thin edge, should be made, upon a clean glass plate [about 10 cm. (3.94 ins.) square], from cement paste of normal consistency.

49.—*Normal Test.*—A pat is immersed in water maintained as near 21° C. (70° F.) as possible for 28 days, and observed at intervals. A similar pat, after 24 hours in moist air, is maintained in air at ordinary temperature and observed at intervals.

50.—*Accelerated Test.*—A pat is exposed in any convenient way in an atmosphere of steam, above boiling water, in a loosely closed vessel, for 5 hours. The apparatus recommended for making these determinations is shown in Fig. 6.

51.—To pass these tests satisfactorily, the pats should remain firm and hard, and show no signs of cracking, distortion or disintegration.

52.—Should the pat leave the plate, distortion may be detected best with a straight-edge applied to the surface which was in contact with the plate.

REPORT OF COMMITTEE E ON PRESERVATIVE COATINGS FOR IRON AND STEEL.

The work of the Committee has followed the lines presented in the last annual report in continuation of the service test on the Pennsylvania Railroad bridge over the Susquehanna River at Havre de Grace.

The analysis of the fifty-seven paints applied to the nineteen sections of the bridge has been completed. The results were obtained by two independent analysts, Mr. P. H. Walker and Mr. P. C. McIlhiney, two members of the Committee who were directly in charge of the work. Mr. Walker's report is embodied in Appendix I, and the results obtained by Mr. McIlhiney are presented in Appendix II to this report (Plates IV, V and VI). The analyses were conducted in a very thorough and complete manner and give a clear indication of the composition of the pigment. The results obtained on the volatile part of the vehicle also give satisfactory indications of the composition. The results obtained from the analysis of the fixed oils, however, fail to give as much information as is desired relative to the quality, and in some instances even the nature of the oil. The constants which are the basis of conclusions are so modified by the oxides in the japan and in some cases by combination with the pigment as to be nearly valueless. These points will be fully detailed in report on analysis of the paints.

The result of the last official inspection made in May, 1908, is given in the following report of the Chairman of the Sub-Committee on Inspection:

PHILADELPHIA, PA., June 1, 1908.

MR. S. S. VOORHEES,
Chairman. Committee E, American Society for Testing Materials,
Washington, D. C.

Dear Sir:—

Comparison of independent notes of inspection of Havre de Grace Bridge on May 4, shows that the large majority of the paints are, to date, affording fair protection. The films are generally hard, elastic and adhesive, but noticeably lacking in lustre or gloss, with the following marked exceptions;

Paint 1. The gloss is markedly high; other characteristics as generally noted.

Paint 2. The film is badly fissured and cracked when observed under a glass, indicating early disintegration.

Paint 10. Higher gloss than generally noted, though very slight in comparison with No. 1. The film is brittle and shows tendency to alligator.

Paint 14. The film is lustreless but hard and brittle; badly cracked, showing lower coat. This feature is very noticeable on the lower chord bars of the bridge and particularly on the north side.

Paint 15. The film is failing seriously in nearly all panels though less on No. 600 than on the others. The bridge proper shows unmistakable evidences of rust with marked failure of the coating.

The general remarks relative to panels are applicable to the bridge proper. When anything is noticeably different between panels and bridge, it is noted.

(Signed) W. A. AIKEN,
Chairman, Sub-Committee on Inspection.

As was anticipated, no marked differences are noted in the majority of cases. As was clearly indicated at the inspection a year ago, the only example of an asphaltum coating thinned with a petroleum volatile solvent has failed to a marked degree after eighteen months exposure. A carbon paint containing rosin in the oil has developed minute fissure cracks all over the surface. While it is by no means conclusive that this failure is due to the rosin, still it is well worthy of note, and will be the subject of further investigation.

An example of difference in expansion of a red lead under-coat and a carbon final-coat is shown in Panel 14. The carbon coat has cracked badly on the bridge proper in cobweb-like cracks showing the red of the under-coat. The same final-coat applied also as an under-coat has not failed in this manner.

It is also interesting to note that one of the pure red lead pigments in straight raw linseed oil shows unmistakable evidence of alligatoring.

High gloss and tenacious film is shown by Panel 1. In this case the pigment consists of oxide of iron, red lead, and a carbon black mixed with a varnish containing some gum and thinned with turpentine.

The other panels or sections, as stated in the report of the Sub-Committee, show in most cases a marked loss of gloss with

minor differences in hardness, tenacity, toughness, and elasticity of film. These differences are, however, too slight to warrant expressing an opinion at this time.

There is evidence of rusting, to a slight degree, on all sections of the bridge, due to mechanical injury. These spots have been accurately noted and will be carefully watched at subsequent inspection. In general, however, the paints are affording good protection and it will require longer exposure to differentiate in the majority of cases. Still, each year will eliminate one or more of the paints as showing marked signs of failure.

The report of the Director of Tests, who was personally in charge of the work, is attached as Appendix III to this report. It shows the method followed in cleaning the surface and applying the paint, and the method used to determine the amount of paint applied per unit of surface, together with the Director's personal observations relative to the test. This report will be of much value in conjunction with the detail sheets giving data on surface and application of paint to each section. It will also be used in connection with reports of inspection.

It is becoming more and more evident that some form of laboratory accelerated tests are required to give indications of the value of a preservative coating. Individual members of the Committee have given this subject most serious consideration.

It is felt that the problem should be attacked by investigating the causes of destruction, the function of both the pigment and the vehicle, and the changes occurring during oxidation. It will be of much value to make a micro-photograph of the film in the condition resulting from normal application to the surface. Also micro-photographs of thin smears of the pigment will show clearly the relative size of the pigment particles. It will also be necessary to measure, and if possible express in relative or even actual terms, the hardness, tensile strength, elasticity, permanent set, adhesion, permeability, etc., of the film both *in situ* and detached from the surface.

One great difficulty has been the preparation of the film suitable for comparison. So far it has been impossible to prepare films from different paints containing different percentages of pigment and vehicle, so that they will be of comparable thickness. As the properties mentioned are largely a function of the vehicle,

the results will not be comparable with varying amounts of pigment.

It is felt that valuable data can be obtained by comparison of the results after different conditions of exposure. Investigations of this character, to be of value, must be conducted in the most painstaking and time-consuming manner, and it has been impossible for any member of the Committee to devote to this investigation the necessary time. The importance of the subject is more than sufficient, however, to warrant continued investigation, and it is hoped that by the next annual meeting, your Committee can report some practical method of making laboratory accelerated tests.

The following letter was received from the Secretary of the Paint Manufacturers' Association:

PHILADELPHIA, October 26, 1907.

MR. S. S. VOORHEES,
Chairman, Committee E, American Society for Testing Materials,
Washington, D. C.

Dear Sir:—

Doubtless you are familiar with the paint tests undertaken at the request of the Bureau of Promotion and Development of the Paint Manufacturers' Association of the United States, by Professor E. F. Ladd, and the North Dakota Agricultural College, at Fargo.

At a meeting of this Bureau held in Chicago on October 24, 1907, it was decided to duplicate the North Dakota tests at various points in the country with a view to determining the modifying effects of different climates, etc. One station selected for these extra tests was Atlantic City, N. J., and it is proposed that Committee E shall accept the supervision of these tests in the same manner as the North Dakota tests were accepted by the Agricultural College of that state.

It is desired that the Atlantic City test shall begin as soon as possible, and certainly this fall. If Committee E will accept the proposition to supervise the tests, and to assume jurisdiction over them during their continuance, the Bureau of Promotion and Development will pay all expenses connected therewith, including the supplying of the paints, analyses, erection of the fence, application, etc. As the time is very short, prompt action is necessary. Therefore, will you kindly take the matter up at once with the various members of the Committee by mail and ascertain whether they are willing to accept the responsibility suggested. My vote, of course, is in the affirmative and I have already informed the Bureau that I think the Committee will be quite willing to do what is asked of them. I have also communicated with Mr. Robert Job, who, I believe, thinks very favorably of the proposition.

It is proposed to either rent a piece of ground at a proper situation for the fence, or to ask the Pennsylvania Railroad Company, who doubtless own a great deal of land in the vicinity of Atlantic City, to allow us to erect a fence somewhere on their property. I am writing Dr. Dudley personally regarding the matter, suggesting that he ascertain for me whether such a piece of ground can be secured.

(Signed) G. B. HECKEL,
Secretary.

This offer was very carefully considered by the members of the Committee. It was recognized that such a test was much needed. It was felt, however, that these tests should be planned, developed and controlled entirely by Committee E, if the Society was to be responsible for the result. This was not possible, as the Paint Manufacturers' Association had prepared the paint on the formulas determined by them and had them ground in the special oils and japans desired. It was finally decided by the majority of the Committee to accept the supervision of the application of the paints, and a sub-committee was appointed with the understanding that its chief duties were to see that the paints prepared by the Paint Manufacturers' Association were fairly applied and to give reports in its several subsequent inspections only as to the relative appearance of the various panels. The first report of this Sub-Committee is as follows:

MR. S. S. VOORHEES,
Chairman, Committee E, American Society for Testing Materials,
Washington, D. C.

Dear Sir:—

By your direction a sub-committee composed of the following: Messrs. Aiken, Heckel, Sabin, P. H. Walker, and Robert Job (Chairman), met December 27, at the offices of Booth, Garret and Blair, Philadelphia, to decide whether or not it was desirable that Committee E should undertake the inspection of the painting of test panels which were to be placed upon a test fence at Atlantic City by the Paint Manufacturers' Association of the United States. By vote of the Sub-Committee Mr. J. F. Walker was also appointed a member and elected Secretary, and the Chairman of Committee E was elected a member of the Sub-Committee, ex-officio.

At the meeting Mr. Heckel, Secretary of the Paint Manufacturers' Association, stated that its object was to duplicate the tests made by Commissioner Ladd upon the North Dakota fence, with the same formulas there used as well as others which the Association wishes to test. Atlantic City was chosen for the exposure in order to get severe condi-

tions due to moist salt air and sand blast action and uneven temperature ranges. Mr. Heckel also stated that the paints for test had already been prepared by the Paint Manufacturers' Association at different factories using linseed oil and driers there on hand. The oil in each case was commercially pure linseed oil.

The Sub-Committee was further advised that the painting was to be upon specially prepared and dried panels of white pine, yellow pine, and cypress, respectively, and that the painting and drying was to be in a room under even temperature in order to avoid the unfavorable winter weather conditions. All of the panels had been given a careful inspection for the Association by a professional lumber inspector, and the effort had been made to get all in as nearly as possible the same condition.

Each paint was to be known only by number, and the test was to be not at all a contest of manufacturers, but was merely to determine any characteristic differences between the different formulas under the special exposure conditions.

It was the wish of the Association that the work of inspection be undertaken by Committee E in order that no question might arise regarding the perfect fairness in the application of the paints.

The fence was to extend north and south. Duplicate panels were to be placed upon each side of the fence, each paint was to be applied to panels of each of the above mentioned woods, and in the painting the weight of the paint applied to each panel was to be carefully determined.

All expenses incurred by Committee E in the inspection would be paid by the Association, including the expenses of the members of the Sub-Committee in attending meetings and inspection trips duly called by the Chairman.

The Association agreed to furnish to Committee E the formula of each paint, with composition of both pigment and drier.

In the course of a thorough discussion of the subject by the Sub-Committee, it was pointed out that under the conditions named by the Paint Manufacturers' Association, Committee E could merely assume to see that the paints prepared by the Paint Manufacturers' Association were applied impartially, giving reports in subsequent inspections as to the relative appearance of the panels. No responsibility could be taken regarding the composition of formulas, nor would the Committee be in position to draw conclusions officially from the results of the exposure.

On this understanding it was voted to accept for Committee E the inspection of the painting, and to recommend to the Chairman of Committee E the appointment of a sub-committee to take charge of the work. A report was made to this effect, and the Chairman of Committee E appointed the same sub-committee to take charge of the matter. Mr. J. H. Parthree of the Wilmington shops of the Pennsylvania Railroad was appointed by the Sub-Committee as its official inspector to be on hand during the entire progress of the painting to see that the work was carried out in a thoroughly impartial manner, and to take the weight of the paint applied to each panel. Mr. Parthree has certified that their conditions

were faithfully maintained throughout the work, and the statement of the weight of paint applied at each coat upon each panel is in the hands of the Sub-Committee.

Three coats were applied to each panel at intervals of about two weeks between each coat, and the panels were finally placed in position on the fence.

A general committee meeting was held at the offices of Booth, Garrett and Blair on January 31, 1908, and one inspection trip was made by the Sub-Committee on May 5; another is planned during the progress of the annual meeting of this Society in June and others thereafter, as often as may seem desirable to the Sub-Committee. It is hoped that in the course of the annual meeting, the fence will be visited by the members of Committee E and by all who are interested in the test.

The formula and composition for each panel, with key, has been furnished each member of the Sub-Committee, and one of the cans of each paint and each japan used, is in charge of the Chairman, in case checking any analysis should seem desirable. Also, to facilitate the work of inspection, blanks have been printed, as per sample herewith, to be filled out by each member of the Sub-Committee for each panel, at each inspection trip.

At the request of the Sub-Committee, the Paint Manufacturers' Association will also institute a series of check tests in which the white-paint base of each formula used will be applied in triplicate at three different rates of spreading, using the same oil and the same thinner in each case.

Also, after the exposure tests are finished, the most durable formula is to be taken, the drier varied, and exposures made.

A report has recently been printed by the Scientific Section of the Paint Manufacturers' Association, giving a detailed statement regarding the test fence. In that report an inaccurate statement appeared regarding the connection of Committee E with the tests. The matter was brought to the attention of the Paint Manufacturers' Association at our inspection trip on May 5, and a corrected statement to accord with our present report was prepared and given to the Association, which agreed to publish it in the form of a slip to be sent to every person to whom their report had been sent. In the future any statement issued by the Paint Manufacturers' Association concerning the work of Committee E will first be submitted to the Sub-Committee for approval. In this manner it will be possible to avoid inaccuracies.

The Sub-Committee believes that valuable indications will be given through this work of the Paint Manufacturers' Association, and it is hoped that at a future time Committee E may be in position financially to supervise throughout a similar test in order that the entire operation including the preparation of the paints, and all details, may be wholly under control which will be recognized everywhere as thoroughly disinterested, as well as scientific.

(Signed) ROBERT JOB,
Chairman.

The report of this Sub-Committee clearly defines the position of Committee E in this series of tests. Committee E's supervision extends only to seeing that these paints are applied in a fair and impartial manner following the method adopted by Prof. Ladd in the North Dakota series of tests, and includes the inspection of the panels at the subsequent inspections.

As the Committee did not determine the formulas on which the paints were made, nor follow the manufacture of these paints, our responsibility covers only the application and subsequent inspection.

A set of tests similar to the series started at Atlantic City is being carried on at Pittsburg. The results from these two separate investigations, together with the results now being obtained from the test fence erected by the Paint Manufacturers' Association at Fargo, North Dakota, will undoubtedly give this Committee some very valuable indications for further investigation along these lines.

Respectfully submitted on behalf of the Committee,

S. S. VOORHEES,
Chairman.

J. F. WALKER,
Secretary.

Appendix I.

PAINT ANALYSIS.

By Percy H. Walker.

The series of paints with which this paper deals were all intended as protective paints for iron and steel. The samples were delivered at the laboratory in three lots, the first lot, Nos. C 3476 to C 3492, inclusive, were received May 8, 1907. The second lot, Nos. 3677 to 3697, inclusive, were received August 7, 1907, and the third lot, Nos. 3913 to 3932, inclusive, were received November 2, 1907. Labels on the cans indicated that the samples were all drawn between the middle of August and the middle of October, 1906. The first lot was completed August 10, 1907, the second lot, October 2, 1907, and the third lot January 10, 1908. About seventeen months, therefore, elapsed from the time of taking the sample until the completion of the analyses.

The samples as received were in quart Mason jars and as a rule were in apparently good condition. The jars were handled with as little shaking as possible and notes were made as to condition when received. The jars with their contents were weighed and when possible, the clear vehicle was drawn off. The jars were then again weighed, the contents transferred to other vessels, the jars wiped out and again weighed. These weighings gave the data necessary to figure the original contents of the jars.

The methods of analysis were in the main alike in all three series, but some modifications were suggested as the work progressed, and the methods used in the second series were more satisfactory than those used in the first, and those used in the third series were better than those used in the second. In the tables, Plates I, II and III, "(*a*)" gives the description of the samples as received in the laboratory, and the percentages of vehicle and pigment. These were determined by treating a portion of the well mixed residue left after drawing off as much as possible of the vehicle, with solvents. A continuous extraction apparatus was not used but the pastes in amounts varying from 7 to 75 grams were shaken with the sol-

vents, allowed to settle, the solvent decanted and the residue shaken with a fresh portion of the solvent. At first four portions of about 250 c.c. each of 88° gasoline were used to extract the vehicle. Samples 3476-84-85-86-87-88-89-90-91 and 92 were treated with this solvent alone. The other samples were treated with gasoline as above and then washed once with benzol and once with ether. Attempts were made to extract first with ether but the pigment does not settle and it is generally better to begin with gasoline and then use benzol and ether. With gasoline the first settling usually takes about twelve hours, the subsequent settlings being more rapid, usually less than two hours when using gasoline, but settling with other solvents always takes more time than with gasoline. Samples 3481, 3484, 3485 3683, 3684, 3685, 3692, 3693 and 3694 were not sufficiently settled to draw off any vehicle. The samples were thoroughly mixed and samples taken for the extraction. Nos. 3683, 3684, and 3685 were all liquid; the others all contained pigment in amounts varying from 10.9 per cent. to 77.9 per cent. Samples 3922 and 3926 were dry colors.

Where separation was possible the amounts of vehicle secured varied from 45 to 363 grams. In some cases this was clear, and in others more or less cloudy. In the first series volatile oils were determined by simply distilling amounts varying from 41 to 108 grams. The apparatus used was a 200 c.c. Jena glass Erlenmeyer flask which was connected with a vertical condenser by a tube bent twice at right angles, all connections being made of cork. The Erlenmeyer flask was suspended in a small air bath carrying a thermometer and heated by a Bunsen burner. The distillation was carried on until no further liquid came over when the thermometer in the air bath registered 190° C. The volatile liquid was caught in a small weighed separatory funnel which was held on the bottom of the condenser by a cork having a small notch cut in it to act as a vent. The volatile liquid in the separatory funnel was weighed and allowed to settle clear, the small amount of water was then drawn off and the funnel was again weighed. The remaining volatile oil was subjected to the tests indicated in the tables. In calculating the percentage of mineral oil from the unpolymerized residue it was assumed that 6 c.c. of pure turpentine would show not more than 0.2 c.c. of unpolymerized residue. These percentages are, of course, only rough approximations.

ALYF

(...YES OF PIGMENTS.

Soly- ized 6 cc.	D.	$(AlFe)_2O_3$.	ZnO.	Pb_3O_4.	$PbSO_4$.	White lead.	SO_3.	Serial No.
	ent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	
2	ce	73.23	....	9.67	....	....	1.46	C3476
ne	ne	54.94	None	6.48	....	....	....	C3477
o	ce	0.55	None	None	None	None	None	C3478
2	e	6.22	None	None	None	None	None	C3479
o	e	6.17	None	None	None	None	None	C3480
2	e	.37	None	None	None	None	None	C3481
8	e	3.35	32.25	None	18.37	27.01	None	C3482
....	...	35.62	2.67	....	....	....	31.25	C3483
2	5	.46	....	6.92	....	....	....	C3484
2	e	.28	....	5.90	....	....	Trace	C3485
4	4	.34	....	7.31	....	....	Trace	C3486
7	e	.46	....	26.84	....	....	Decided	C3487
2	e	.42	....	27.45	....	....	Trace	C3488
2	e	.36	....	27.81	....	....	Trace	C3489
....		2.48	....	65.12	....	....	.43	C3490
....		2.39	....	56.20	....	....	.42	C3491
o	e	3.41	....	2.80	....	....	.57	C3492

ANALYSES OF BRIDGE PAINTS—I

(a) DESCRIPTIO[illegible]

Serial	[illegible]	Coat No	Date taken	Conditions when received in Contracts Laboratory	Pigment	[illegible]	Specific gravity at 20° C	Water	Volatile oils
			1906		Per cent	Per cent		Per cent	Per cent
C3477	1	1	Aug 17	Well settled, pigment red filled over half of jar, vehicle very dark reddish	39 0	61 0	0 9770	[illegible] 2	21 0
[illegible]	1	2	Sept 18	Line of demarcation not clear pigment black, oil black	20 1	79 9	9732	[illegible]	19 0
[illegible]	2	1	Aug 17	Line of demarcation not clear, pigment black, oil somewhat lighter in color than C3477	35 1	64 9	9230	2	11 7
[illegible]	2	2	Sept 18	Pigment dark gray, filled half of jar, vehicle dark	35 3	64 7	9232	Trace	10 5
[illegible]	2	[illegible]	Oct 2	Pigment dark gray, filled half of jar, vehicle dark	35 5	64 5	9231	1	11 7
C3481	3	1	Aug 13	No separation, very black	10 9	[illegible] 1		5	53 1
C3482	3	2	Sept 18	Well settled, pigment gray, filled over half of jar, vehicle clear like linseed oil	61 6	[illegible] 4	9131	1	18 4
C3483	3	3	Oct 2	Well settled, pigment dark brown, filled two thirds of jar, vehicle clear like linseed oil	41 9	[illegible] 1	9375	1	2
C3484	4	1	Aug 13	No separation, very black	11 2	[illegible] 8		3	3[illegible] 0
[illegible]	4	2	Sept 19	No separation, very black	12 8	[illegible] 2		5	23 4
[illegible]	4	3	Oct 2	No separation, very black	1[illegible] 7	[illegible]		6	30 0
[illegible]	5 [illegible]	1	Aug 22	Pigment black, filled four-fifths of jar, vehicle dark brown, clear	52 5	47 5	9535	9	[illegible] 4
[illegible]	5 [illegible]	2	Sept 19	Pigment black, filled four-fifths of jar, vehicle dark	52 4	47 6	9563	9	[illegible] 2
[illegible]	5 [illegible]	3	Oct 2	Pigment black, filled four fifths of jar vehicle, dark	52 0	48 [illegible]	9464	7	7 7
[illegible]	6	1	Aug 22	Pigment light red; filled two thirds of jar, vehicle dark	55 9	44 1	1 0307	(c)	(c)
[illegible]	6	2	Sept 19	Only slight separation, pigment dark brown	59 4	40 6	1 0176	(c)	(c)
[illegible]	6	3	Oct 2	Pigment black, filled three-fourths of jar, vehicle dark	26 5	73 5	9378	3	11 8

(b) ANALYSES OF VEHICLE

ANALYSIS OF VOLATILE OILS — ANALYSIS OF NON-VOLATILE VEHICLE

Serial	Specific gravity at 20° C	Flash point	Refractive index at 20° C	Boiling point	Unpolymerized from 6cc	Mineral oil calculated from unpolymerized	Ash	Manganese in ash	Lead in ash	Lime in ash	Magnesium in ash	Reaction of ash	Rosin by L and S reaction	Saponification No.	Acid No.	Iodin No.
		° C		° C		Per cent	Per cent			Per cent						
C3477	0 8075	42	1 4602	150	0 2	None	2 75	Present	Present	Trace	None	Faintly alkaline	Absent	110 1	14 1	144 7
[illegible]	8070	45	1 4714	[illegible]	None	None	1 85	Present	Present	1 29	None	Alkaline	Absent		[illegible] 3	
[illegible]	7474	Room temp	1 410[illegible]	11[illegible]	3 0	85	85	Present	Present	11 77	None	Alkaline	Present		[illegible] 2	
[illegible]	7392	Room temp	1 4170	115	[illegible] 2	80	1 12	Present	Present	9 33	None	Alkaline	Present		11 2	
[illegible]	7400	Room temp	1 4180	115	[illegible] 0	80	92	Present	Present	10 90	None	Alkaline	Present		11 1	
C3481	8026	41	1 4693	150	2	None	[illegible] 09	Present	Present	None	None	Neutral	Absent		9	
C3482	8150	Room temp	1 4105	[illegible]	8	60	25	Present	Present	None	None	Neutral	Absent	190	[illegible] 8	137 6
C3483							12	Present	Present	None	None	Neutral	Absent	225 8	[illegible] 2	171 9
C3484	8007	40	1 4700	150	2	None	91	Present	Present	Trace	None	Neutral	Absent		[illegible] 8	
[illegible]	8032	4[illegible]	1 4603	155	2	None	1 02	Present	Present	Trace	Trace	Neutral	Absent		6	
[illegible]	8022	45	1 4696	150	4	[b] 5	1 29	Present	Present	Trace	Trace	Neutral	Absent		[illegible] 3	
[illegible]	8074	Room temp	1 44[illegible]0	12[illegible]	7	40	1 05	Present	Present	Trace	None	Faintly alkaline	Absent	182 [illegible]	1[illegible] 9	107 5
[illegible]	8146	Room temp	1 4482	135	2	35	2 22	Present	Present	95	Trace	Neutral	Absent	181 8	14 9	133 3
[illegible]	8187	Room temp	1 1471	1[illegible]	[illegible] 2	15	2 20	Present	Present	1 11	Trace	Neutral	Absent	186 3	11 3	148 2
[illegible]							10 62	Present	Present	42	Trace	Faintly alkaline	Absent	158 0	0	140 7
[illegible]							8 98	Present	Present	61	Trace	Faintly alkaline	Absent	141 0	1 1	
[illegible]	8470	Room temp	1 45[illegible]8	14	5	10	71	Present	Present	0 30	None	Neutral	Absent	198 3	4	150 3

(c) ANALYS[illegible]

Serial	Undetermined	Carbonaceous matter	Mineral matter insoluble in HCl	Character of mineral matter insoluble in HCl	CaO	MgO	[illegible]	[illegible]	P[illegible]	$PbSO_4$	Water[illegible]	[illegible]	Serial
	Per cent	Per cent	Per cent		Per cent	Per cent	[illegible]	[illegible]	Per cent	[illegible]	[illegible]	[illegible]	
C3477	[d] 2 03	15 44	1 57	Silicate Fe	[illegible]	Tra[illegible]			[illegible]			[illegible]	[illegible]
[illegible]	[e] 2 96	34 58	1 24	Fe_2O_3 and silicate	None	None		None	[illegible]				[illegible]
[illegible]	[d] 4 64	[illegible] 75	[illegible] 56	Al silicate and SiO_2	None	Trace		None	[illegible]	[illegible]	[illegible]		
[illegible]	[d] 4 03	33 70	53 06	Al silicate and SiO_2	[illegible]	None		[illegible]	[illegible]	[illegible]			
[illegible]	[d] 5 31	33 81	54 71	Al silicate and SiO_2	None	None		[illegible]	[illegible]				
C3481	[e] 1 15	94 74	3 72	[illegible]$AlFe_2O_4$	None	None	[illegible]	[illegible]	[illegible]				
C3482	[d] [illegible] 92	1 44	1[illegible]	SiO_2AlFe	None	None		[illegible]	[illegible]				[illegible]
C3483	[d] [illegible]	[illegible] 16	54	SiO_2AlFe	21 02		[illegible]	[illegible]				[illegible]	
C3484	[e] 5 54	85 45	65	Silicate	[illegible] 8	0 1	[illegible]		[illegible]				[illegible]
[illegible]	[e] 6 60	86 [illegible]	44	Silicate	[illegible]	Trace							
[illegible]	[e] 7 73	82 30	1 2	Silicate	[illegible] 6	2[illegible]			[illegible]				
[illegible]	[e] 2 12	67 [illegible]	[illegible] 33	Silicate	1 12	Trace			[illegible]				
[illegible]	[e] 2 16	60 [illegible]	2 32	Silicate	1 11	Trace			[illegible]				
[illegible]	[e] 1 47	67 13	[illegible] 60	Silicate	57	Trace			[illegible]				
[illegible]	[d] 5 32	[illegible] 0	21 20	Mg silicate	[illegible]	85			[illegible]				
[illegible]	[d] 3 85	14 50	10 14	Mg silicate	[illegible]	75			[illegible]				[illegible]
[illegible]	[d] 1 27	82 28	5 10	Ca silicate	[illegible] 48	Trace	[illegible]		[illegible]				[illegible]

a C3481 appears to contain asphalt
b Doubtful
c Very small amount, lost determination

d Mainly water
e Contains volatile organic matter
f Nos 3484, 3485, and 3486 contain bituminous substance similar to soft coal

no figı

ALYSI TS.

ORTION		Soluble in HCl.						
om sion	B p.	CaO.	MgO.	SO_3.	P_2O_5.	SiO_2.	Total.	Serial No. Contracts Labora-tory.
	c	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.		
5		0.61	1.35	None	None	0.92	100.12	3677
o		.53	1.50	None	None	.06	100.13	3678
o	1	.59	1.38	None	None	.69	99.71	3679
.....		None	None	None	None	None	98.96	3680
.....		None	None	None	None	None	99.61	3681
.....		None	None	None	None	None	99.46	3682
	10	None	None	None	None	None	100.00	3683
	10	None	None	None	None	None	100.00	3684
	10	None	None	None	None	None	100.00	3685
	5	Trace	.37	....	Trace	....	100.17	3686
	6	Trace	.58	....	Trace	....	99.84	3687
	5	Trace	.61	....	Trace	....	99.78	3688
1	No	1.35	....	.67	0.07	....	100.01	3689
1	No	.74	....	.63	.07	....	99.96	3690
1	No	1.40	....	.67	.07	....	99.58	3691
1	5	1.51	Trace	3.39	.35	....	99.37	3692
1	5	.70	Trace	3.60	.18	....	99.60	3693
1	6	.57	Trace	3.91	.20	....	99.58	3694
......	....	Trace	....	Trace	....		99.96	3695
.....	....	.04	....	Trace	....		99.24	3696
.....	....	.15	....	Trace	....		98.68	3697

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ANALYSES OF BRIDGE PAINTS—II.

[Where no figures are given not enough material was available for determination]

(a) DESCRIPTION — (b) ANALYSES OF VEHICLE.

Serial No. Contracts Laboratory	Panel No.	Can No.	Date taken	Condition when received in Contracts Laboratory.	Pigment	Vehicle.	Specific gravity of vehicle at 20° C	Volatile by dry dist	Volatile oil in vehicle.	Non-volatile in vehicle.	Analysis of Volatile Portion: Specific gravity at 20° C.	Refractive index at 20° C	Flash at room temperature	Residue from polymerization of 6 cc: One-half hour	Residue from polymerization of 6 cc: One hour.	Boiling point	Approximate mineral oil	Ash
			1906		Per cent	Per cent		Per cent	Per cent	Per cent				cc	cc	° C	Per cent	Per cent
[illegible]	1	1	Sept 6	Well separated, odor of turpentine, vehicle like linseed in color, pigment black	48.8	51.2	0.9420	2.6	10.8	89.2	0.8651	1.4060	No	0.55	0.65	15	5	0.48
[illegible]	1	2	Sept 29	Well separated, odor of turpentine, vehicle like linseed in color, pigment black	50.0	[illegible]	.9439	1.3	10.2	89.8	..	1.4002	No	45	60	1	5	.48
[illegible]	13	[illegible]	Oct 6	Well separated, odor of turpentine, vehicle like linseed in color, pigment black	36.9	[illegible]	.9441	5.0	10.3	89.7	.8426	1.4074	No	50	70	1	5	.44
[illegible]	14	1	Sept 9	Well separated, odor doubtful (turpentine?); vehicle cloudy, pigment bright red	76.2	23.8	1.0488	7	3.1	96.9		1.4735						10.82
[illegible]	14	2	Sept 29	Poor separation, vehicle and pigment both black, no odor of turpentine	16.0	84.0	.9340		Trace only									.03
[illegible]	14	3	Oct 9	Poor separation, vehicle and pigment both black, faint odor of turpentine	18.8	81.2	.9131	6	2.6	97.4								.02
[illegible]	15	1	Sept 11	No separation, very black, gasoline odor		100.0	No separation	Not made	a 38.3		.7308	1.4120	Yes	5.70	5.[illegible]	75	100	
[illegible]	15	2	Sept. 29	No separation, very black, gasoline odor		100.0	No separation	Not made	a 40.5		.7341	1.4174	Yes	5.80	5.[illegible]	75	100	
[illegible]	15	3	Oct 9	No separation, very black, gasoline odor ..		[illegible]	No separation	Not made	a 37.5		.7368	1.4172	Yes	5.90	6.[illegible]	95	100	
[illegible]	16	1	Sept 11	Fair separation, odor of turpentine, vehicle like linseed oil in color, pigment black	29.1	70.[illegible]	.9130	12.4	16.2	83.8	.7800	1.4457	Yes	[illegible].10	3.2[illegible]	110	50	.08
[illegible]	16	2	Sept 29	Good separation, odor of turpentine, vehicle like linseed oil in color, pigment black	31.5	68.5	.9144	11.0	15.7	84.3	.7660	1.4312	Yes	[illegible].80	3.[illegible]	110	60	.05
[illegible]	16	[illegible]	Oct	Good separation, color of turpentine, vehicle like linseed in color, pigment black	32.9	67.1	.9173	11.1	16.6	83.4	.7883	1.4394	Yes	[illegible].20	3.[illegible]	110	50	.04
[illegible]	17	1	Sept 17	Good separation, odor of turpentine, vehicle like linseed oil in color, pigment reddish brown	55.7	44.3	.9319		11.4	88.6	.8669	1.474[illegible]	[illegible]	20	[illegible]	1.6	None	.65
[illegible]	17	2	Sept 29	Good separation, color of turpentine, vehicle like linseed oil in color, pigment reddish brown	54.2	45.8	.9339		10.7	89.3	.8680	1.4736	No	20	[illegible]	1.7	None	.92
[illegible]	17	[illegible]	Oct	Good separation, color of turpentine, vehicle like linseed oil in color, pigment reddish brown	57.7	42.3	.9328	2.3	12.2	87.8	.8654	1.4743	No	[illegible]	[illegible]	1.7	None	1.04
[illegible]	18	1	Sept 18	No separation, odor of turpentine, black	14.8	85.2	No separation	Not made	a 5.7		.7858	1.4309	Yes	3.6[illegible]	3.7[illegible]	115	55	
[illegible]	18	2	Sept 29	No separation, odor of turpentine, black	13.1	86.9	No separation	Not made	a 3.0		.7808	1.4362	Yes	3.5[illegible]	3.7[illegible]	115	55	
[illegible]	18	[illegible]	Oct	No separation, odor of turpentine, black	11.1	88.9	No separation	Not made	a 3.6		.7834	1.4307	Yes	3.80	3.9[illegible]	115	60	
[illegible]	19	1	Sept 15	Poor separation, odor of turpentine, vehicle dark, contains some pigment, pigment black	14.7	85.3	.9337		4.2	95.8		1.4712						.06
[illegible]	19	2	Sept 29	Poor separation, color of turpentine, vehicle dark, contains some pigment, pigment dark	14.7	85.[illegible]	.9324		6.3	93.7		1.4744						.09
[illegible]	19	[illegible]	Oct	Poor separation, color of turpentine, vehicle dark, contains some pigment, pigment black	15.6	84.4	.9329		3.6	96.4		1.4743						.17

ANALYSIS OF NON-VOLATILE PORTION — (c) ANALYSES OF PIGMENT

Panel No.	Can No.	Reaction of ash.	Composition of ash	Iodin No.	Acid No.	Unsaponified matter	Rosin by L & S	L[illegible] [illegible]° C	Loss on ignition less carbonaceous matter weighed as such	Carbonaceous matter	Insoluble in HCl: Insoluble mineral matter.	Insoluble in HCl: Nature of mineral matter	Al_2O_3, Fe_2O_3	Pb_3O_4	Mn	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	Serial No. Contracts Laboratory
						Per cent		Per cent	Per cent	Per cent	Per cent		Per cent	Per cent		[illegible]	Per cent	Per cent	Per cent	Per cent		
1	1	Neutral	Mn, Pb, trace Ca	140.3	8.0	0.09	None	[illegible]	b 2.83	35.46	58.74	Aluminum silicate	8.75	0.52	None	[illegible]	[illegible]	None	None	[illegible]	[illegible]	[illegible]
1	2	Neutral	Mn, Pb, trace Ca	148.3	8.5	.27	None	[illegible]	b 3.11	25.24	59.38	Aluminum silicate	8.09	1.01	None		[illegible]	None	None	[illegible]		[illegible]
13	[illegible]	Neutral	Mn, Pb, trace Ca	138.9	9.5	.39	None	[illegible]	b 2.51	25.03	58.59	Aluminum silicate	[illegible]	[illegible]	None		[illegible]	None	None	[illegible]	[illegible]	[illegible]
14	1	Alkaline	Mn, Pb, trace Ca	137.1	36.2	.41	None	[illegible]	None	None	19.07	Aluminum silicate	None	79.[illegible]	None		[illegible]	None	None	None	[illegible]	
14	2	Neutral	Mn, Pb, no Ca	138.1	7.7	.35	None	[illegible]	10.51	86.24	.27		None	2.38	None	[illegible]	None	None	None	None	[illegible]	[illegible]
14	3	Neutral	Mn, Pb, no Ca	140.9	7.4	.45	None	[illegible]	12.20	85.19	None		None	1.75	None	[illegible]	None	None	None	None	99.[illegible]	[illegible]
15	1							[illegible]		[illegible]	.65	SiO_2	None	None	None	[illegible]	[illegible]	None	None	[illegible]	[illegible]	
15	2							[illegible]		58.11	1.40	SiO_2	None	None	None		[illegible]	None	None	None	[illegible]	[illegible]
15	3							[illegible]		[illegible]	1.42	SiO_2	None	None	None		[illegible]	None	None	None	[illegible]	
16	1	Neutral	Mn, Pb, trace Ca	139.6	9.4	.35	None		b 2.08	47.52	17.44	Mg, Al, SiO_2	[illegible]	1.18		[illegible]			Trace		[illegible]	
16	2	Neutral	Mn, Pb, trace Ca	144.4	7.6	.33	None	[illegible]	b 2.29	45.79	1[illegible].35	Mg, Al, SiO_2	[illegible]	1.23		[illegible]			Trace		[illegible]	[illegible]
16	[illegible]	Neutral	Mn, Pb, trace Ca	144.6	11.6	.37	None	[illegible]	b 2.61	[illegible].10	47.76	Mg, Al, SiO_2	[illegible]	1.[illegible]			[illegible]		Trace		[illegible]	
17	1	Neutral	Mn, Pb, trace Ca	154.0	7.2	.41	None	[illegible]	1.77	9.74	36.56	$BaSO_4$, 33.30, SiO_2, 2.06	40.05		Trace			.67	0.07		[illegible]	
17	2	Neutral	Mn, Pb, trace Ca	150.3	8.3	.51	None	.14	1.41	9.34	38.23	$BaSO_4$, 36.10, SiO_2, 2.13	40.4[illegible]	...	Trace	[illegible]		.63	.07		99.9[illegible]	
17	[illegible]	Neutral	Mn, Pb, trace Ca	141.[illegible]	7.7	.13	None	[illegible]	1.42	9.75	36.42	$BaSO_4$, 34.12, SiO_2, 2.00	40.6[illegible]		Trace	[illegible]		.67	.07		99.[illegible]	[illegible]
18	1							1.[illegible]	d 6.85	69.39	1.84	Ca, Al, SiO_2	3.12	11.09		[illegible]	Trace	3.39	.33		[illegible]	[illegible]
18	2							1.[illegible]	d 6.58	70.28	5.87	Ca, Al, SiO_2	.55	10.40		[illegible]	Trace	3.60	.18		99.6	[illegible]
18	[illegible]							1.10	d 4.09	70.68	6.85	Ca, Al, SiO_2	.84	11.28		[illegible]	Trace	3.91	.20		[illegible]	[illegible]
19	1	Neutral	Mn, Pb, no Ca	155.5	10.3	.06	None	1.[illegible]	d 4.76	80.23	.96	Mg, Al, SiO_2	1.09	1.26			Trace		Trace		[illegible]	[illegible]
19	2	Neutral	Mn, Pb, no Ca	150.0	12.4	.22	None	[illegible]	d 5.30	85.92	.71	Mg, Al, SiO_2	1.12	[illegible]			1		Trace		[illegible]	[illegible]
19	[illegible]	Neutral	Mn, Pb, trace Ca	155.2	7.3	.15	None	[illegible]	d 6.08	84.86	.87	Mg, Al, SiO_2	.93	1.17			.15		Trace		9[illegible].68	[illegible]

a No separation of pigment and vehicle volatile matter figured on weight of mixed paint
b When rubbed, polishes like graphite On heating for "Loss on ignition," a little "volatile" matter formed, the samples burned very slowly like graphite
c Apparently simply coal tar dissolved in mineral oil Could not extract with any solvent Analysis represents whole paint
d When rubbed, does not give good polish (little luster). On heating for loss on ignition formed a lot of flame like volatile combustible matter in coal (crucible had to be covered to prevent loss). Afterwards carbon burned off rapidly

A[illegible] OF VEHICLE.

[illegible]proximate mineral oil.	Ash.	R[illegible] c	Al_2O_3.	Fe_2O_3.In.	Excess S.	Total.	Pb_3O_4 by titration.	PbO calculated.	Serial No. Contracts Laboratory.
Per cent.	Per cent.		Per cent.	Per cen					
a 75	0.76	N[illegible]	....	......ace	i 0.19	98.99	....	......	3913
a 75	.74	N[illegible]	....	......ace	i .01	99.42	......	......	3914
a 75	.79	N[illegible]	....	......ace	i .07	100.04	......	......	3915
15	1.26	Ne	....	......ace	......	99.45	......	......	3916
20	1.28	Ne	....	......ace	......	99.64	......	......	3917
15	1.29	Ne	....	......ace	......	99.75	......	......	3918
60	.12	Ne	21.78	13.22 ace	......	98.43	......	......	3919
60	.13	Ne	19.72	12.07 ace	......	99.89	......	......	3920
60	.16	Ne	18.98	10.92 ace	......	99.83	......	......	3921
....	....	...	....	......ne	......	99.12	88.52	10.33	3922
....	10.14	Neu	....	......ne	......	99.30	91.73	7.08	3923
....	11.28	Neu	....	......ne	......	98.81	90.84	6.77	3924
....	12.04	Neu	....	......ne	......	98.99	86.61	8.50	3925
....	....	...	....	......ne	......	99.52	95.89	3.51	3926
....	2.52	Neu	....	......ne	......	99.55	95.62	3.61	3927
....	2.55	Neu	....	......ne	......	99.63	95.89	2.99	3928
....	1.72	Neu	....	......ne	......	99.49	93.64	4.14	3929
(d)	.25	Alk[illegible]	....	......ne	......	100.52	......	......	3930
Doubtful. d	f 7.28	Fai[illegible] alk[illegible]	....	......ace	......	100.32	......	......	3931
Doubtful. d	f 5.18	Fai[illegible] alk[illegible]	....	......ace	......	100.33	......	......	3932

[illegible]gh to test. [illegible]ot so good as samples
[illegible]1, and 3932 contain acetone.
[illegible], and 3932 do not give the i[illegible]c matter and careful
3932 contained suspended pi[illegible]ry matter extracted [illegible]
[illegible]d, as loss of lead or zinc ren[illegible] 5.04 in No. 3932.
[illegible]rned off very slowly. Gave
[illegible]ur is sulphide sulphur in exc[illegible]

PLATE [illegible]
AM. SOC. T[illegible]
VOLUME VIII
[illegible] COMMITTEE
[illegible]

ANALYSIS OF VEHICLE

Description	Specific gravity at 15.5° C	Volatile oil in vehicle	Non-volatile in vehicle	Specific gravity at 15.5° C	Refractive index at 15.5° C	Flash at room temperature	Residue from polymerization: One-half hour	One hour	Boiling point	Approximate mineral oil	Ash	Reaction of ash	Composition of ash	Iodine No.	Acid No.	Unsaponified matter	Rosin by L & S	Specific gravity	Loss at 105° C	Organic matter	Carbonaceous matter	Insoluble mineral matter	Nature of mineral matter	Alkali
		Per cent	Per cent							Per cent	Per cent					Per cent			Per cent	Per cent	Per cent	Per cent		Per cent
[illegible] clear, [illegible] fine black, turpentine odor doubtful	[illegible]62	8.5	91.5	[illegible]	1.4[illegible]	Yes	[illegible]			[illegible]	0.70	Neutral	Pb, Mn, tr Zn, tr Ca	171.9	9.2	2.02	None	0.9586	0.05	(a)	[illegible]	49.01	Al silicate, 25.74 BaSO4, 23.97	[illegible]
[illegible] clear, dark brown, well settled, sediment fine black, turpentine odor pungent	[illegible]15	10.5	89.5	.7941	1.4[illegible]	Yes	4[illegible]			[illegible]	.74	Neutral	Pb, Mn, tr Zn, tr Ca	167.2	[illegible]	.70	None	[illegible]17	.12	(a)	[illegible] 36.73	42.57	Al silicate, 24.[illegible] BaSO4, 17.77	1.5[illegible]
[illegible] clear, dark brown, well settled, sediment fine black, turpentine odor pungent	.9321	10.5	89.4	.7924	1.4[illegible]	Yes	4.4	[illegible]		[illegible]	.72	Neutral	Pb, Mn, tr Zn, tr Ca	169.1	5.0	.61	None	[illegible]	.15	(a)	4[illegible].17	47.8[illegible]	Al silicate, 27.[illegible] BaSO4, 20.36	1.0[illegible]
Clear colored oil, well settled, sediment dark, slight turpentine odor	[illegible]175	12.6	87.4	.8414	1.4[illegible]	[illegible]	7			[illegible]	1.20	Neutral	Pb, Mn, tr Zn, no Ca	[illegible]	[illegible]	.50	None	[illegible]	.06	0.47	[illegible] 12.67	4.15	Like carborundum	[illegible]
Clear colored oil, well settled, sediment dark, slight turpentine odor	.9372	15.5	[illegible]	.8486	1.46[illegible]	[illegible]	[illegible]			[illegible]	1.25	Neutral	Pb, Mn, tr Zn, no Ca	155.2	[illegible]	.18	None	[illegible]	.05	.28	[illegible] 92.02	4.13	Like carborundum	[illegible]
Oil cloudy, not so well settled, otherwise like [illegible]	.9453	12.6	87.4	.8426	1.46[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	1.23	Neutral	Pb, Mn, tr Zn, no Ca	150.1	7.7	.14	None	[illegible]	.07	.38	[illegible] 92.84	4.42	Like carborundum	[illegible]
Oil clear, light amber, well settled, sediment brownish black, turpentine odor doubtful	[illegible]	[illegible]	91.7	.7546	1.4[illegible]	Yes	[illegible]		[illegible]	[illegible]	.12	Neutral	Pb, Mn, tr Zn, tr Ca	170.0	[illegible]4	.25	None	[illegible]	.18	5.92	[illegible] 13.84	34.1[illegible]	Al silicate,	
Oil clear, light amber, well settled, sediment has about 10 per cent of red material as bottom layer, turpentine odor doubtful	[illegible]16	4.1	95.9	.77[illegible]	1.4[illegible]	Yes	7		[illegible]	[illegible]	.13	Neutral	Pb, Mn, tr Zn, tr Ca	167.1	[illegible]4	.29	None	[illegible]	.38	8.41	[illegible] 17.67	4[illegible].35	Al silicate	
Oil black, not well settled, sediment black, turpentine odor doubtful	.9144	4.3	95.7	.7581	1.4[illegible]	Yes		4	[illegible]	[illegible]	.16	Neutral	Pb, Mn, tr Zn, tr Ca	170.3	[illegible]	.19	None	[illegible]	.44	7.27	[illegible] 24.22	[illegible]4	Al silicate	
Dry color																			None	(a)	Trace	None		None
Oil light color, slight amount of red matter in suspension, well settled, sediment red and very hard, odor like peanut	[illegible]199	None	10[illegible]								10.14	Neutral [b]	Pb, no Mn, tr Zn, tr Ca	157.1	22.0	.53	None		None	[illegible] 2.39	.30	None		None
Like 1925, except sediment reddish brown	[illegible]536	None	[illegible]								11.25	Neutral [b]	Pb, no Mn, tr Zn, tr Ca	[illegible]	22.0	.44	None		None	[illegible] 2.52	.07	None		[illegible]
Light colored, jelly, carbon in suspension, sediment reddish brown, black on one side of can (very thin coat)	[illegible]200	None	[illegible]								12.64	Neutral [b]	Pb, no Mn, tr Zn, tr Ca	[illegible]	14.2	.58	None		None	[illegible] 5.52	3.12	[illegible]		.45
Dry color																			.02	(a)	Trace	None		None
Oil clear, reddish, sediment hard like red lead, odor like linseed, well settled	[illegible]	None	[illegible]								2.52	Neutral	Pb, Mn, tr Zn, tr Ca	[illegible]	14.7	.11	None		.01	(a)	.22	None		None
Oil thicker than 1927, also darker in color, residue also darker, odor like linseed, well settled	[illegible]682	None	[illegible]								2.65	Neutral	Pb, Mn, tr Zn, tr Ca	[illegible]	12.7	.14	None		None	(a)	.17	None		None
Oil like 1928, residue on one side like 1928, other side black (very thin coat), well settled	[illegible]	None	[illegible]								1.72	Neutral	Pb, Mn, tr Zn, tr Ca	[illegible]	12.1	.17	None		None	(a)	1.7	None		None
Oil clear, reddish brown, linseed odor, sediment like color, well settled	[illegible]	5	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	.25	Alkaline	Pb, Mn, tr Zn, Ca	1[illegible].9	[illegible]	Abt [illegible]	None [illegible]	[illegible]	.18	[illegible] 5.51	Trace	[illegible].57	SiO_2	1.96
Oil black from suspended matter, separation [illegible], sediment like tar, slight odor of turpentine	[illegible]	[illegible]	[illegible]	[illegible]	1.4036	No			[illegible]	Doubtful [d]	[illegible].25	Faintly alkaline	Pb, Mn, tr Zn, tr Ca			Abt [illegible]	None [illegible]		.7[illegible]	[illegible] 12.18	69.10	.13	Fe_2O_3, [illegible]	1.14
Like 1931	[illegible]	[illegible]	92.1	[illegible]	1.4696	No	[illegible]			Doubtful [d]	[illegible].18	Faintly alkaline	Pb, Mn, tr Zn, tr Ca			Abt [illegible]	None [illegible]		.93	[illegible] 12.51	65.97	.46	Fe_2O_3, SiO_2	1.26

ANALYSES OF PIGMENT

[illegible]

[a] [illegible] test.
[b] [illegible] and 1932 contain acetone.
[c] [illegible] and 1932 do not give the [illegible] in CS_2 test for [illegible].
[d] [illegible] 1932 contained suspended pigment, so figures are higher.
[illegible] as loss of lead or zinc rendered results worthless.
[e] Carbon burned off very slowly. Gave good polish like graphite.
[f] This [illegible] is sulphide sulphur in excess of that combined with zinc [illegible].

[illegible] burned off very slowly [illegible].
[h] Not counted in summation [illegible].
[i] Included in summation as [illegible].
[m] Included in summation but not [illegible]. [illegible] of this material 11.37 per cent was recovered in No. 1931 as [illegible] matter.

At first the plan used in the first series was followed in the second series, but it was found that though fairly good results were obtained when the vehicle contained large amounts of volatile oils, very bad results were obtained where little volatile oil was present. This throws a good deal of doubt on the percentage of volatile oils in the first series, the figures being probably low. The method then followed was to distil about 25 grams in a current of live steam, using a 500 c.c. round-bottom flask with a spray trap between the flask and condenser. Distillation was continued until 100 c.c. of water had passed over. The oils were then separated from water and weighed and 0.3 gram added, a correction arbitrarily chosen for the oil dissolved in the water. The testing of the volatile oils was done on the mixed oils obtained from dry and steam distillation, using, of course, different portions of the vehicle; but in examining the non-volatile portion, the residue from the dry distillation alone was used. In this series there are six samples which had not settled; in these the volatile oil was determined by steam distillation.

In the third series the determination of volatile and non-volatile in the vehicles was effected by a method which is better than any used in the first two series. Amounts varying from 27 to 79 grams were weighed into a 500 c.c. round-bottom flask connected with a condenser by means of a spray trap. A current of live steam was passed through until 100 c.c. water collected in the receiver the oils were separated from water, weighed, and the same correction (0.3g.) applied as in the other steam-distillation method. After driving off the volatile oils the steam was cut off and air drawn through. At the same time the flask was heated to 130° C. In about 15 minutes all water was driven off and the residue was used for testing the non-volatile portion. Only three samples (3930–31–32) of the third series were subjected to dry distillation, and these were so treated in attempting to determine acetone, which, while it was indicated by shaking the paint with water and getting the iodoform test on the watery solution, could not be quantitatively determined, since no distillate could be recovered even on heating to 190° C.

The tables of analyses show clearly what was done with the non-volatile portion of the vehicle. In the first series saponification numbers were determined on several of the samples. In the last

two series this was not done, but the unsaponifiable matter was determined instead.

In the analyses of the pigments in the first series the samples were treated with hydrochloric acid (1:1), filtered on a weighed paper, dried at 105° C., and weighed. The residue was then ignited and again weighed. The loss on ignition was called carbonaceous matter.

In the second and third series a different plan was followed. One portion was weighed out and the loss at 105° C. determined. This was then ignited and again weighed. The second loss would be combined water, carbonaceous matter, CO_2, etc. Another portion was treated with hydrochloric acid, filtered on a Gooch crucible, dried at 105° C. and weighed. This was then ignited and the loss was called "carbonaceous matter." This subtracted from the total loss gives the "loss on ignition less carbonaceous matter weighed as such."

The rest of the results were obtained in the ordinary way. In the first and second series the lead was assumed to be as red lead and calculated as Pb_3O_4 except in No. 3483 where it is calculated as lead sulphate and white lead, the sulphate being figured from the SO_3 found, and the remaining lead calculated as basic carbonate, a qualitative test showing CO_2.

In the third series, Nos. 3913 to 3921, inclusive, the lead is calculated as PbO. In Nos. 3922 to 3932, inclusive, lead is calculated as Pb_3O_4. On the red lead paints, Nos. 3922 to 3929, inclusive, in addition to determination of total lead, which for the summation was calculated as Pb_3O_4 the amount of actual Pb_3O_4 was determined by titration of iodine liberated from KI by the pigment in the presence of large excesses of sodium acetate.

It must be confessed that these analyses do not give the information desired. We can with reasonable certainty decide whether the volatile oil used was turpentine or mineral oil, we can come to a fairly good conclusion as to the composition of the pigment, though even here we are frequently left in the dark. For example, we cannot say whether the magnesium silicate found was of an absestos or talc nature. These, however, might possibly be determined by the microscope. On the whole, for determining the general nature of the inorganic pigments and the volatile oils used, chemical analysis is fairly satisfactory. The carbonaceous

matter present, however, may be due to carbonaceous matter in the original pigment or it may be due to unextracted vehicle. In No. C 3476 the pigment was red. It appeared to have no carbon, but the analysis shows 10.44 per cent. carbonaceous matter. In this case this carbonaceous matter was due to unextracted vehicle (gasoline only was used in washing out the vehicle). Now what was the nature of this unextracted vehicle? Was it linoxyn? From the fact that we have nearly 10 per cent. of red lead present and the remainder is nearly all oxide of iron it is certainly possible. Was it gum? A varnish instead of a straight oil might have been used. From the information available concerning this sample we learn that the last hypothesis is probably the correct one; but it must be confessed that from the chemical data alone we really cannot decide. If we take the cases, however, of the dark colored pigments where some carbonaceous matter is evidently present in the pigment, we certainly cannot say that all of the carbonaceous matter found was derived from the original pigment. While as has been shown the interpretation of the analyses of extracted pigments often presents great difficulty, the interpretation of the analyses of the non-volatile portion of the vehicle is much worse. I must confess that an attempt to draw any conclusions from these analyses is very discouraging, in spite of the large amount of work done. Perhaps it would have been better had we subjected all samples to an analysis adapted to varnishes with the idea of determining the gums present. Such analyses are at best unsatisfactory and we were also of the opinion that most of the samples would at any rate be straight oil paints. The specific gravities of the total vehicle and, in the last series, of the non-volatile portions were determined with a pyknometer. This was a mistake. A Westphal balance should have been used and wou d have practically eliminated the error due in some cases to the small amount of pigment which did not settle out. In some cases these analyses can be interpreted as indicating pure linseed oil, as, for example, No. C 3483; but in other cases this cannot be done. Consider No. 3482. From the ash, saponification number and specific gravity (which by allowing for the 18 per cent. of light oil found in the vehicle would be about 0.935 for the non-volatile vehicle) would indicate pure linseed oil; but the iodine number is low. We know though that on mixing with lead compounds the iodine

number of linseed oil is sometimes lowered very much. We also know that some linseed oils have a specific gravity above 0.935. Now is this a pure linseed oil which has had its iodine number lowered by being in contact with lead carbonate and sulphate, or is it a mixture of linseed oil with other fatty oil? I must confess I cannot say.

This paper is presented with a hope that it will lead to a discussion of the interpretation of paint analysis. I wish to express my thanks to the Assistant Chemists in the Contracts Laboratory, Messrs. F. W. Smither, H. C. McNeil, and E. W. Boughton, for their skilful and careful work in completing this long series of very complex analyses. These gentlemen did all the analytical work.

Panel No.	Coat No.	Per cent. of Pigment.	Per cent. of Oil.	Per cent. Volatile.	White Lead, per cent.	Vehicle. Figure On Saponifiable Oils.	Volatile. Flash at 30° C.	Volatile. Refractive Index at 20° C.	Volatile. Remarks
1	1	36.8	46.5	16.7	....	155.9	No	1.4663	Turps.
1	2	20.8	64.8	15.4	....	....	"	1.4641	"
2	1	33.5	55.6	10.9	....	....	Yes	1.4164	Turps. and benzine.
2	2	39.9	51.4	8.7	....	....	"	1.4142	" "
2	3	36.9	53.7	9.4	....	....	"	1.4171	" "
3	1	6.9	46.5	46.6	....	....	No	1.4653	Turps.
3	2	62.5	28.1	9.4	30.43	....	Yes	1.4470	Turps. and benzine.
3	3	48.3	49.3	2.4	....	....	No	1.4590	Turps.
4	1	14.0	60.1	25.7	....	....	"	1.4652	"
4	2	7.9	71.2	20.9	....	....	"	1.4650	"
4	3	15.2	59.6	25.2	....	....	"	1.4653	"
5	1	52.0	44.2	3.8	....	....	Yes	1.4527	Turps., benzine and camph. oil
5	2	47.5	45.6	6.9	....	....	"	1.4465	" " "
5	3	50.7	46.7	2.6	....	....	"	1.4482	" " "
6	1	61.1	35.7	3.2	....	....	No	1.4191	Turps. and benzine.
6	2	60.2	34.9	4.9	....	....	...	....	"
6	3	34.0	57.7	8.3	....	....	Yes	1.4593	Turps. and benzine.
7	1	52.6	42.5	4.9	....	....	"	1.4226	Turps., benzine and camph. oil
7	2	52.8	41.1	6.1	....	....	"	1.4263	" " "
7	3	52.8	41.7	5.5	....	....	"	1.4219	" " "

PLATE IV.
PROC. AM. SOC. TEST. MATS
VOLUME VIII.
REPORT OF COMMITTEE E.
APPENDIX II.—P. C. MCILHINE.

ANALYSIS OF BRIDGE PAINTS—I

					PIGMENT.													VEHICLE								
																					Koettstörfer Figure				Volatile	
Panel No.	Coat No.	Per cent. of Pigment.	Per cent. of Oil.	Per cent. Volatile.	White Lead, per cent.	Red Lead, per cent.	Sulphate of Lead, per cent.	Zinc Oxide, per cent.	Lithopone, per cent.	Silica, per cent.	Clay, per cent.	Oxide of Iron, per cent.	Graphite, per cent.	Carbon or Lampblack, per cent.	Barytes, per cent.	Silicates, per cent.	Gypsum.	Acid Figure.	Per cent. Unsaponifiable.	Hard Gums.	On Vehicle.	On Non-Volatile Oil.	On Saponifiable Oils.	Flash at 30° C.	Refractive Index at 20° C.	Remarks.
1	1	36.8	46.5	16.7		9.88						78.94						11.0	3.3	16.32	111.0	150.8	155.9	No	1.4663	Turps.
1	2	20.8	64.8	15.4		5.28						53.60		36.30				12.3		10.50		115.8		"	1.4641	"
2	1	33.5	55.6	10.9						41.18		17.00		34.60				12.4				179.8		Yes	1.4164	Turps. and benzine
2	2	39.9	51.4	8.7						39.20		16.60		41.20				14.4				136.1		"	1.4142	" "
2	3	36.9	53.7	9.4						53.30		10.04		37.20				10.4			158.0	186.3		"	1.4171	" "
3	1	6.9	46.5	46.6										96.00				1.0						No	1.4653	Turps.
3	2	62.5	28.1	9.4	30.43		15.51	25.62			13.80							4.9			146.3	195.3		Yes	1.4470	Turps. and benzine
3	3	48.3	49.3	2.4								33.80					64.76	1.1			180.2	189.0		No	1.4590	Turps.
4	1	14.0	60.1	25.7		4.40								91.80							..	....		"	1.4652	"
4	2	7.9	71.2	20.9		4.58								90.24				5.4			....	....			1.4650	"
4	3	15.2	59.6	25.2		2.40								88.22				8.3			125.5	178.7		"	1.4653	"
5	1	52.0	44.2	3.8		22.04								68.26				13.2			176.0	191.3		Yes	1.4527	Turps., benzine and camph. oil
5	2	47.5	45.6	6.9		28.50								68.70				8.8			174.9	201.5		"	1.4465	" " "
5	3	50.7	46.7	2.6		27.38								66.62				8.3			169.5	179.0		"	1.4482	" " "
6	1	61.1	35.7	3.2		71.53				19.22								21.8			135.4	149.6		No	1.4191	Turps. and benzine.
6	2	60.2	34.9	4.9		5.91				13.20				21.40				19.6			144.8	165.3			...	"
6	3	34.0	57.7	8.3										78.85				8.4			168.6	182.9		Yes	1.4593	Turps. and benzine.
7	1	52.6	42.5	4.9					28.10		21.53			30.51				6.8			171.1	190.9		"	1.4226	Turps., benzine and camph. oil
7	2	52.8	41.1	6.1					20.98		24.91			49.60				8.1			168.7	193.9		"	1.4263	" " "
7	3	52.8	41.7	5.5					27.83		20.13			41.94				6.8			164.5	180.3		"	1.4210	" " "

					VEHICLE.					
								Volatile.		
Panel No.	Coat No.	Per cent. of Pigment.	Per cent. of Oil.	Per cent. Volatile.	White Lead, per cent.	Red Lead, per cent.	Oils.	Flash at 30° C.	Refractive Index at 20° C.	Remarks
8	1	33.7	57.4	8.9	....	..	..	No	1.4560	Turps.
8	2	31.8	59.5	8.7	....	..	..	...	....	
8	3	34.8	56.3	8.9	....	..	..	No	1.4549	Turps.
9	1	40.4	56.4	3.2	....	..	.7	Yes	1.4288	Turps. and benzine.
9	2	38.4	58.4	3.2	....	..	..	...	....	
9	3	26.0	71.0	3.0	....	..	.9	Yes	1.4344	Turps. and benzine.
10	..	Dry	Pigm	ent	....	100	..	...	....	
10	1	74.8	24.6	0.6	....	100	.2	...	....	
10	2	65.8	23.9	0.3	....	100	.2	...	....	
10	3	58.3	41.1	0.6	....	100	..	...	....	
11	..	Dry	Pigm	ent	....	100	..	...	....	
11	1	78.8	20.9	0.3	...	100	..	...	....	
11	2	77.3	22.4	0.3	....	100	..	No	1.4638	Turps.
11	3	76.5	23.4	0.1	....	100	..	...	....	
12	1	54.6	42.4	3.0	18.64	..	..	...	....	
12	2	6.6	90.3	3.1	....	13	..	No	1.4637	Turps.
12	3	7.1	88.3	4.6	....	14	..	"	1.4648	"
13	1	49.9	44.7	5.4	....	..	..	"	1.4453	"
13	2	51.2	43.2	5.6	....	..	..	...	....	"
13	3	49.4	45.1	5.5	....	..	..	No	1.4579	"

PLATE V.
PROC. AM. SOC. TEST. MATS.
VOLUME VIII.
REPORT OF COMMITTEE E.
APPENDIX II.—P. C. McILHINEY

ANALYSIS OF BRIDGE PAINTS—II.

					Pigment													Vehicle						Volatile		
																					Koettstörfer Figure					
Panel No.	Coat No.	Per cent. of Pigment.	Per cent. of Oil.	Per cent. Volatile.	White Lead, per cent	Red Lead, per cent.	Sulphate of Lead, per cent.	Zinc Oxide, per cent.	Lithopone, per cent.	Silica, per cent.	Clay, per cent.	Oxide of Iron, per cent.	Graphite, per cent.	Carbon or Lampblack, per cent	Barytes, per cent	Silicate, per cent.	Gypsum.	Acid Figure	Per cent. Unsaponifiable.	Hard Gums.	On Vehicle.	On Non-Volatile Oil.	On Saponifiable Oils	Flash at 30° C.	Refractive Index at 25° C	Remarks
8	1	33 7	57 4	8.9									97 80					0 9			163 9	189 4		No	1 4500	Turps.
8	2	31 8	59.5	8.7									95 73					7.3			167 9	192 3				
8	3	34.8	56.3	8.9									96.84					5 6			161 8	187 6		No	1 4549	Turps.
9	1	40 4	56.4	3 2									10 16			80.70		4 0	0 69		146 7	154 9	147 7	Yes	1 4288	Turps. and benzine.
9	2	38 4	58.4	3.2									31 02			70.10		5 4			163 9	172 9				
9	3	26 0	71.0	3.0									41.00			53 39		7.3	4 90		181 5	168 8	172 9	Yes	1 4344	Turps and benzine.
10		Dry	Pigment			100 00																				
	1	74 8	24 6	0.6		100.00												22.0	6 70		168 8	158 2	169 2			
	2	65 8	23.9	0.3		100.00												22 4	15 35		157 9	151 0	177 2			
10	3	58.3	41.1	0.6		100 00												32 6			150.0					
11		Dry	Pigment			100.00																				
11	1	78 8	20.9	0.3		100.00												7 1			188 0	192 0				
11	2	77.3	22 4	0.3		100.00												8.9			186.0	213 0		No	1 4638	Turps
	3	76.5	23.4	0.1		100 00												8.7			191.9	188 9				
12	1	54 6	42.4	3.0	18.64			12.20						16.14		47.10		2.7			197 6	188 5				
12	2	6.6	90.3	3 1		13.77								81 10				8.8						No	1 4637	Turps.
12	3	7.1	88.3	4.6		14 55								80 53				11.0						"	1 4648	"
13	1	49.9	44 7	5.4							57 96	8 17	24 62					4.6			174.9	195.9		"	1 4453	"
13	2	51 2	43 2	5 6							55 80	9 78	29 76					5.3			167.1	188 8				"
	3	40.4	45 1	5.5							50.20	17.94	30 00					5 6			175.5	196 9		No	1 4579	"

PLATE VI.
PROC. AM. SOC. TEST. MATS.
VOLUME VIII.
REPORT OF COMMITTEE E.
APPENDIX II.—P. C. MCILHINEY.

Panel No.	Coat No.	Per cent. of Pigment.	Per cent. of Oil.	Per cent. Volatile.	Vehicle. White Lead, per cent.	Vehicle. Oils.	Volatile. Flash at 30° C.	Volatile. Refractive Index at 20° C.	Remarks.
14	1	77.4	21.4	1.2	....	1.6	...	....	Turps.
14	2	8.6	89.7	1.7	....	..	No	1.4660	"
14	3	8.4	88.0	3.6	....	..	...	....	
15	1	17.7	46.3	36.0	....	..	Yes	1.4016	Benzine.
15	2	....	....	....	....	..	"	1.4072	"
15	3	....	....	....	....	..	"	1.4050	"
16	1	29.5	56.3	14.2	....	..	"	1.4297	Turps. and benzine.
16	2	28.5	58.2	13.3	....	..	"	1.4273	" "
16	3	32.9	54.2	12.9	....	..	"	1.4324	" "
17	1	54.8	37.8	7.4	....	..	No	1.4670	Turps.
17	2	55.0	40.4	4.4	....	..	...	....	
17	3	53.5	40.6	5.9	....	..	No	1.4663	Turps.
18	1	13.3	80.2	6.5	....	..	Yes	1.4298	Turps. and benzine.
18	2	14.0	81.7	4.3	....	..	"	1.4283	" "
18	3	10.2	85.7	4.1	....	..	"	1.4288	" "
19	1	11.4	83.1	5.5	....	..	No	1.4410	Turps.
19	2	17.4	79.5	3.1	....	..	"	1.4653	"
19	3	10.9	85.9	3.2	....	..	"	1.4658	"

ANALYSIS OF BRIDGE PAINTS—III.

					PIGMENT.													VEHICLE			Koettstörfer Figure					Volatile.
Panel No	Coat No.	Per cent. of Pigment.	Per cent. of Oil.	Per cent. Volatile.	White Lead, per cent	Red Lead, per cent	Sulphate of Lead, per cent.	Zinc Oxide, per cent.	Lithopone per cent	Silica, per cent	Clay, per cent.	Oxide of Iron, per cent.	Graphite, per cent.	Carbon or Lampblack, per cent.	Barytes, per cent.	Silicate, per cent.	Gypsum, per cent.	Acid Figure.	Per cent. of Unsaponifiable.	Hard Gums.	On Vehicle.	On Non-Volatile Oil	On Saponifiable Oils	Flash at 3[illegible]° C	Refractive Index at 2[illegible]° C	Remarks
14	1	77.4	21.4	1.2	...	79.99		.	...	...	18.46	...		.		...	...	27.7	8.7	...	156.7	165.6	171.6	.	...	Turps
14	2	8.6	89.7	1.7	.	.	.		....	....	..	...	.	89.66		....	...	5.6	..	...	205.0	206.0	..	No	1.4660	"
14	3	8.4	88.0	3.6	....	..	...			...	.	.		98.30	..	...	...	7.2	.		175.1	188.6	..	...	..	
15	1	17.7	46.3	36.0		..			...	.	...	.		99.38		..	...	.			...	...	.	Yes	1.4016	Benzine.
15	2	..	.....	...	.		.		.		...			99.52		..	...	.				...	.	"	1.4072	"
15	3	.	..	.	.	.			.	.	.		..	99.12								.	..	"	1.4050	"
16	1	29.5	56.3	14.2	.	...		..		.	.			50.0[illegible]		46.10		7.5			149.5	187.1	.	"	1.4297	Turps. and benzine
16	2	28.5	58.2	13.3	.	.		..						48.14		47.70	.	7.7			148.0	182.2	.	"	1.4273	" "
[illegible]	[illegible]	32.9	54.2	12.9				...	.	.				54.00	.	4[illegible]		6.2			154.9	191.9	..	"	1.4324	" "
17	1	54.8	37.8	7.4					.			49.32		7.84	34.64	...	..	2.9			169.3	202.5		No	1.4670	Turps.
17	2	55.0	40.4	4.4			.	..	.		.	49.65		9.20	36.00	.	....	7.1			174.6	193.5		...	...	
17	3	53.5	40.6	5.9					.			48.06	.	1.50	34.68	.	...	3.5		.	176.5	201.9		No	1.4663	Turps.
	1	13.3	80.2	6.5			14.03	.						69.80			.	14.4	.		190.3	205.7		Yes	1.4298	Turps. and benzine
18	2	14.0	81.7	4.3			10.77		..		..			81.72		..	..	...		.	178.2	187.6		"	1.4283	" "
18	3	10.2	85.7	4.1		.	10.08				.			75.16		.	....	13.5	...	...	158.9	166.5		"	1.4288	" "
19	1	11.4	83.1	5.5		4.06								93.08			..	8.8	..	...	206.2	209.8	.	No	1.4410	Turps.
19	2	17.4	79.5	3.1		3.38								93.50			.	14.4			181.6	188.7	.	"	1.4653	"
19	3	10.9	85.9	3.2		4.89		..						90.32				2.5		..	119.1	123.5		"	1.4658	.

Appendix III.

SUPPLEMENTARY REPORT OF THE DIRECTOR OF TESTS.

The application of the paint was completed on October 15, 1906, and all data required by the Committee was submitted by the Director of Tests on a tabulated sheet for each separate manufacturer's protective coating. In answer to the Committee's request for descriptive notes as to conditions and incidents relating to the work, the following supplementary report is respectfully submitted:

The structure had been completed but a few months, and the painting was the first following the shop and erection coat. For the test the panels were numbered from west to east, making twenty panels for the two spans, and Panels 1 to 19 were used for the tests. Panel 20 was turned over to the contractor as his regular work, and his paint was applied on Panel 20 at the same time as that of the test on Panel 19. The division of the panels for the tests was so made that each of the 19 tests was applied to a full panel cross section of the bridge, but in order to give each test a full truss-post the dividing line was drawn half way between posts.

As the Committee required each test to be made with three coats, while some of the manufacturers asked for a test of two coats only, a compromise was made by painting the entire down stream half of the 19 panels, three coats, and allowing two coats *only* on the up stream half of such panels as the manufacturers resquested.

For cleaning the arrangement of staging and men was as follows:

1. Two men, on planks that were extended from floor beam to floor beam, for cleaning the upper work between stringers and between top chords and stringers.

2. A four man staging was worked in each panel between floor beams from the top of floor beams down to the bottom horizontal bracings.

3. Two men on a plank suspended below the bottom chords from truss to truss, moving the plank forward as work progressed.

4. Two men on the outside of the top chord on a plank slung from the outside guard timbers.

The brush men followed on the same kind of stagings except that all the top work was painted from the four man, track suspended, staging. Unfortunately, this arrangement of stagings required the application of two different tests at the same time on the same four man staging, one from each end; and as the areas to be covered were not uniform, one end crew would have more surface to cover than the other. This, in a measure, makes void a comparison of time and areas covered, and furnished encouragement for a man to slight his work in trying to keep up with the other men.

From the beginning of preparations in June, 1906, to September 1, 1906, when work had advanced as far as the cleaning of Panels 1 to 7, and the application of the first coat on Panels 1 to 6, Mr. F. H. Cubberly, from the Philadelphia Department of Bridges, was in charge as Director of Tests. His leave of absence expiring on September 1, the writer assumed the position, taking over the work and continuing it without interruption, except from weather conditions, until completed. The brush work on the bridge was completed October 15, 1906, and the triple sets of test plates were, on November 1 to 3, placed for exposure by fastening them securely in a strong framework that had been previously bolted on the top of the bottom chord of the down stream truss. Each coating-test plate was placed in a vertical position with a southeasterly exposure in its respective bridge-panel.

The Director, after September 1, did not find it necessary to delay the brush work and use the stagings for inspection, but was almost constantly with the men. For both cleaning and brush work he was able to point out errors as the work advanced, and the men were sent back or the staging changed at once to correct these errors. This practice, with the point gained by keeping each man on his particular part of the work, soon put the entire force in line, and while the work was not up to the Committee's anticipated point of theoretical execution, the result was above the average of that attained in general railroad bridge painting. Occasionally, paint was spilled on work partly dry or men used tools not as directed; some in hurrying to keep up with the other better man did not brush out their work well, especially where the mixture worked stiff and the experienced man was better able to do quick work well. In this connection it was demonstrated that a section painted with a stiff mixture appeared uneven and poorly done, when with a light easy flowing mixture a more even and better class of work could be done, regardless of any effort the Director might make to prevent bad work. The above is equally true with regard to the work of different men cleaning the steel.

Weather observations were recorded at the hours designated. If humidity conditions were close to the limit, frequent observations were taken. Sudden light showers would call for a halt on account of excessive humidity and if the reading returned to the safe limit but moisture could be found on the steel by a handkerchief test, work was delayed until no moisture remained. However, it must be admitted that applying paint on days when the humidity is close to the limit and on other days when the air is clear and dry is not a good procedure for comparative tests.

"In the course of application of each individual sample the Director shall obtain from the workmen general information as to the working qualities of the paint, as well as such other particulars as may suggest themselves to him as useful. In each instance he shall also note particularly the rate of drying and the physical appearance of each coat."*

Opinions expressed by the brush men were contradictory and condi-

* Instructions to the Director of Tests. See Vol. VI., p. 53.

tions were such that no attempt at a comparison of working qualities was made. This can best be taken from the notes of the plate test, made by the Committee's experts from the same paint that was being applied on the bridge.

Before the hour for starting work each morning the Director passed over the bridge and examined the steel for moisture or other irregular conditions. Then the weather records were taken and if conditions were right the foreman was signaled to come for paint. The paint was weighed out to the men in regular brush pots ready for use, marked with metal tags, the Director following the pots to their respective panels, and when work stopped or changed the pots and brushes were returned to the store-room and properly cleaned. Where round pound brushes could not be used flat lamb's-wool swabs were permitted and on the inside of bridge seat chairs and the inside of top chord box girders double round pound brushes on long handles were used.

"The painting of the check panels provided for the Committee shall be under the general supervision of a competent and expert painter but the Director shall make careful note of all details in regard thereto in connection with his notes on the general work."

The steel plates used were prepared as directed and were handled entirely by the expert painter and his assistant. The work of cleaning the plates was done in a shanty near the bridge and the paint applied in another room fitted up exclusively for this work. The paint store-room was a third shanty near the others and all were so arranged that the Director could know that the work was properly done without taking his attention from the bridge work.

The instructions from the Committee's letter of July 30, 1906, were carefully followed.

The steel was cleaned by the advance gang of six men on the stagings as previously noted. First a sharp-edged steel chisel, about $1\frac{1}{2}$ ins. wide, and long enough to be used by both hands if desired, was used to remove everything loose. These chisels were kept sharp and did good execution. After the chisels a very stiff steel wire brush was used to remove the rough loose paint that was left behind the chisels and also to brighten up the rusty surfaces. As is usual in large bridges, the condition of the steel surface varied from a smooth, hard black-oil shop-coat finish on most of the beams and top-chord members to a rough sticky, heavily-coated black-paint surface on nearly all of the eye-bar members of the trusses. The eye bars had evidently been stacked up when the paint was wet and in separating them the paint had either peeled off in a few spots or the wood had adhered in a few places to the steel. Many of the eye-bar members, although painted months before, were (in warm weather) still sticky, and if rubbed with the stiff wire brushes became rough but could not be cleaned by any manipulation of the chisel or brush. Only fire or the sand blast would have cleaned the steel entirely of paint and rust. This condition was also true of a few of the 5-ft. floor beams. These

members were gone over with the sharp chisels only, and the first coat of our paint brushed on over the soft shop coat. In general the top and bottom surfaces of the small horizontal members had entirely lost the shop coat but the surface rust had not become rough from scales. The truss posts and some of the bottom chord members threw off scales of paint and rust under the chisels, showing plainly that the shop coat had been applied under bad conditions. Many of the built members showed a shop coat of black on the plates with red lead under the flange and stiffener angles. Red lead had also been freely used in erection, on the bridge seat chairs and pin connections. The top chord (box girder) had also received a good coating of red lead on the inside. Also, before tracklaying, the upper surface of all top members had been painted, according to the Company's standard requirements, with red lead paint and we were required to do no top surface work. Also, complying with the Committee's final instructions, the inside cleaning of box girders and posts was omitted. Bottom chord members were cleaned (where close together) only by such chisel work as was possible and these surfaces are not a part of the comparative test. The use of the long handle brushes in the top-chord box girders was very unsatisfactory. Much paint (of the light mixtures) was lost by dripping from the brush although some of the men were expert and did good work. After the first coat these surfaces were not painted and no long handle brushes were used as these members had been well painted inside before erection. These conditions are pointed out to indicate the obstacles which prevent uniform work on a structure of this class by the average gang of bridge painters in the regular course of work. We can, however, claim to have done as nearly uniform work and to have painted our part of the structure in as thorough a manner as can be expected under present-day conditions. In considering the different sections, it should be remembered that the tabulated data sheets show that the steel was not in a uniform condition. The amount of rust and scale increased towards the east, due undoubtedly to the fact that yard engines in switching came on the bridge from that end. Also, during the progress of our work construction trains loaded with ashes were switching on the east end and not infrequently showered a fresh coat with water or ashes. It was noticed that although no visible signs of grease had been observed before the first coat was applied, still on some sections after the first coat began to dry, there was unmistakable evidence that the inside of the track stringers directly under traffic, were spattered with grease, probably from passing trains or the jolt of an engine stopping on the bridge. This was observed on two sections and noted on the data sheets.

Weather conditions also prevented uniform results. In one instance, paint was applied on two adjoining sections until five o'clock p. m. under correct weather conditions, but a driving storm came on and while one paint held and was not considerably injured, another, a slow drying paint was badly washed and also injured the fresh paint of the adjoining panel by washing down upon it along the batter post and truss diagonals.

The Director endeavored to follow the Committee's written instructions so far as the average conditions and the skill of the workmen could be made to conform to them. If his interpretation was more practical than some of the members may have desired it must be remembered that only the inside work on the test plates was by skilled workmen and under conditions that were wholly within the Director's control.

From these plates all conditions of spreading and drying rates and nature and condition of film should be determined. After September 1, the contractor and his men gave the Director loyal support in the practical execution of the work, under the Committee's instructions, as they had at that time been interpreted, striving so far as possible, to make the work uniform and to give each coat the same handling, and at the same time to proceed with the job in a manner that could be considered consistent for general first-class work. The bridge work should be judged as a practical test that was attended with the usual accidents and incidents on such work, but with more than the usual care regarding weather conditions and correct brush work. The Director's inspection of the completed work indicated that panels painted with the light easy flowing mixtures were more evenly coated and gave evidence of better results than some of the heavier pigments which had what appeared to be a first class vehicle forming a stiff mixture hard to apply. It is equally true that the only trouble caused by the second or third coat breaking away when applied on very cold steel came from what appeared to be the highest-grade heavy-weight paints.

The data sheets give information as to conditions of steel and show the preparation of the surface to have been not the smooth surface free from the black-oil first coat, as had been the Committee's first expectation.

If the element of personal equation (good or bad work) could be eliminated from our field work, it might be profitable to note the comparison in service between the bridge work on a very bad steel surface and on that of the same paint on test plates being exposed to the same weather conditions. Conditions were such, however, that it is not possible to determine whether peculiar conditions of color, surface, texture or apparent brush marks that might be observed by a careful inspection of the bridge, are due to a failure of the mixture to fulfil the requirements of a good protective coat, or to the fact that this particular condition was caused by one of the many possible accidents or incidents noted in connection with the work. Speaking broadly, the experience of the Director impressed him with the belief that users and manufacturers of protective coatings cannot expect positive or uniform results in the field work while the present careless practice in applying shop coats continues, and that until a successful machine method of applying paints can be found to replace the present uncertain hand labor application, the best mixture will not always produce the best protection.

Respectfully submitted.

HOBERT B. POTTER,
Director of Tests.

REPORT OF COMMITTEE F ON THE HEAT TREATMENT OF IRON AND STEEL.

I have the honor to report that work has been done by Committee F on the following subjects:

"Can Ingotism be Cured by Prolonged Exposure to the Temperature at which Overheating is Cured?"*

"On the Carbon Iron Diagram," by H. M. Howe and C. Offerhaus.

"On the Heat Treatment of Medium Carbon Steel."†

"Experiments on Rolling of Steel. Relation between Grain-Size, Finishing Temperature and Reduction in the Rolls." (Work proceeding.) By William Campbell.

Respectfully submitted on behalf of the Committee,

HENRY M. HOWE,
Chairman.

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* See p. 185.

† Vol. VII., p. 240. *Metallurgie*, 4, 772.

CAN INGOTISM BE CURED BY PROLONGED EXPOSURE TO THE TEMPERATURE AT WHICH OVERHEATING IS CURED?*

By Henry M. Howe, William Campbell, and W. T. Koken.

By ingotism we mean that extremely coarse structure which exists in unannealed ingots and steel castings. The symptom by which it is most readily recognized is the white network of "primaustenoid, the network, spines, and other masses rich in ferrite and therefore poor in carbon which, in hypo-eutectoid steel, persist as undiffused relics of the primary austenite formed"† in the original solidification of the molten steel. A strongly marked white V of this primaustenoid is shown in Fig. 1. It is by the presence or absence o this substance that we decide in the present investigation whether ingotism has been cured, i. e. whether the steel has been "de-ingotized."

Can this ingotism be cured, i. e. can the primaustenoid be destroyed by diffusion, by a prolonged exposure to the relatively low therapeutic temperature Ac_3 at which steel which has simply been overheated, say at 1377°, is refined? An effect of this overheating is to coarsen the network, or bands, or spines of ferrite which envelope or pierce the grains of pearlite in slowly cooled hypo-eutectoid steel. The primaustenoid itself in an unannealed casting looks as if it were nothing more than an exaggerated form of the network of ferrite which we find in steel which has simply been overheated; but the genesis of these two kinds of network is really quite distinct. The primaustenoid forms during the solidification of the molten mass; it forms as a concentration, not of ferrite, but simply of austenite, i. e. of metal slightly poorer in carbon than the molten mass out of which it is born. Primaustenoid is not ferrite, it is a region richer in ferrite than the rest of the mass. But the network formed in overheating is ferrite itself, expelled in cooling from Ar_3 or Ar_{2-3} to Ar_1. Because the

* Contribution from the Metallurgical Laboratory of Columbia University.
† *Bimonthly Bulletin*, Am. Inst. Mining Engineers, July, 1908, p. 464.

effacing of each kind of network is purely a process of diffusion, it might be expected that a sufficiently prolonged exposure to the temperature, Ac_3, which suffices to efface rapidly the network formed in overheating, would also suffice to efface the network of primaustenoid. But in view of the difference in kind between the birth of these two substances, and of the consequent extreme difference in degree between the work which diffusion has to do in the two cases, it might well be true that, in order to cure ingotism, the exposure to Ac_3 would have to be extended to a wholly impracticable length.

Again, if phosphorus or manganese sulphide or both are con-

FIG. 1.—Untreated steel casting, 0.43 per cent. carbon, showing white primaustenoid in a ground-mass of mixed ferrite (light) and pearlite (dark). Magnification, 32 diameters.

centrated in the primaustenoid, its effacement may require their removal by diffusion. This removal may either be slower at Ac_3 than that of pure primaustenoid, or it may be unable to complete itself even on indefinitely long exposure to Ac_3, but may need a materially higher temperature for its completion.

The question as it actually came before us in our tests, therefore, took this form: Is primaustenoid effaced by prolonged exposure to a temperature high enough to efface rapidly the coarse ferrite network caused by overheating?

Procedure. A series of sets were made, each consisting of:

One piece of untreated steel casting;
One piece of the same casting which had been de-ingotized by heating to 1377° C;
One piece of rolled steel of like composition, which had been overheated at 1377° along with the steel casting.

Their composition was as follows:

	C. Per cent.	Si. Per cent.	Mn. Per cent.	P. Per cent.	S. Per cent.
The steel casting...	0.43	0.40	0.78	0.05	0.05
The rolled steel	0.38	..	..	0.06	0.04

FIG 2.—Casting shown in Fig. 1 after a reheating sufficiently high to refine its pearlite, but not to de-ingotize it. The patches of white primaustenoid still remain in a mottled ground-mass of fine pearlite and ferrite. Magnification, 32 diameters.

Of these sets, each was heated to a definite high temperature, and either cooled slowly thence immediately after reaching it, as in the cases represented in Columns 1 to 5 of Table I, or else held there for a considerable length of time, as in the cases represented in Columns 6 to 8 of that table, and then cooled slowly. The heatings followed immediately by slow coolings (Nos. 1 to 9) were made

in a gas forge.* Those in which the steel was held at 840° C. (Nos. 10 to 14) were done in a Sauveur muffle, heated internally by the resistance of a coil of platinum wire. All the heatings were controlled by means of a Le Chatelier thermo-electric pyrometer, using a Siemens and Halske galvanometer.

The critical ranges were as follows:

	Ar 1.	Ac 1.	Ar 2-3
Steel casting........	705°	760°	762°

TABLE I.—EFFECT OF HEAT ON THE STRUCTURE OF (1) AN UNTREATED STEEL CASTING; (2) THE SAME CASTING AFTER BEING DE-INGOTIZED; AND (3) OVERHEATED ROLLED STEEL.

Number.	Heated to T° Max., and Immediately Cooled Thence Slowly.				Number.	Heated to 840° C., Held There, and Then Cooled Slowly.	
	T° Max., or Temperature Reached.	Effect on				Hours Held at 840° C.	Effect on Primaustenoid of Previously Untreated Casting.
		Prim-austenoid of Untreated Casting.	Coarse Ferrite of				
			De-ingotized Casting, Overheated at 1377° C.	Rolled Steel. Overheated at 1377° C.			
1	804° C.	Not effaced	Not effaced	Not effaced	10	2	Not effaced
2	840°	" "	Effaced	Effaced	11	8	" "
3	861°	" "	Effaced, and new network begins	Effaced, and new network begins	12	16	" "
4	1000°	" "	Effaced, and new network begins	Effaced, and new network begins	13	24	" "
5	1059°	" "			14	32	" "
6	1118°	" "					
7	1171°	" "					
8	1180°	" "					
9	1194°	Effaced					

The results, which are condensed in the accompanying Table I may be summarized as follows:

(1) The lower therapeutic temperature, that at which the coarse ferrite network caused by overheating at 1377° C. is effaced rapidly, is between 804° and 840° in this steel, and hence probably a little above Ac_3.

* See Metallurgical Laboratory Notes, H. M. Howe, Figs. 19-20, pp. 48-9.

(2) Ingotism is not cured in this steel, i. e. the primaustenoid network is not effaced, even by an exposure of 32 hours to this temperature. Patches of primaustenoid still remain, somewhat as shown in Fig. 2.

(3) The upper therapeutic temperature at which ingotism is cured rapidly in this steel, is between 1180° and 1194°.

(4) Naturally, the lower therapeutic temperature of the de-ingotized steel casting is substantially that of overheated rolled steel of like composition.

(5) The critical temperatures in the initial heating of an unannealed steel casting are the same as those of that same casting after de-ingotizing.

In order to determine whether the failure of this steel to de-ingotize at 840° is due to its containing phosphorus or manganese sulphide, we propose to make like tests with raw castings free from these substances.

REPORT OF COMMITTEE G ON THE MAGNETIC TESTING OF IRON AND STEEL.

During the past year the Committee has given its attention particularly to preliminary work on the testing of core losses in sheet steel. The investigation conducted has been with a view of studying the various commercial methods of testing sheet steel for core losses, and comparing these tests with the results obtained in completed machines.

The procedure has been as follows: Through the courtesy of the American Rolling Mill Company, a heat of sheet steel was obtained; sheets were selected at random, and from these sheets were cut samples for the different methods. No effort was made to obtain steel of high quality; in fact, an inferior grade was selected in order that the differences in the different methods should be more apparent.

Preparation of Specimens.—From the sheets were first cut large annular rings, 14 ins. in diameter, with a radial depth of $1\frac{1}{2}$ ins. Enough of these rings were selected to give a cross-section of $1\frac{1}{2} \times 1\frac{1}{2}$ ins. From the pieces which were cut from the inner parts of these rings were punched stampings of 4 ins. outside diameter and 3 ins. inside diameter, to form test pieces for the apparatus described in Volume VI, 1906.*

From the sheets were also cut rectangular strips, $1\frac{3}{16} \times 15\frac{1}{2}$ ins., for the Epstein method, and sufficient iron was also obtained to form the core of a standard core type transformer.

Tests on Ring Samples.—The large ring was wound with uniform winding, and tests were made by the wattmeter method, corrections being made for the copper loss. One sample was tested as it came from the mill, and another after carefully annealing in the ovens of one of the large electric manufacturing companies. The loss in watts per pound at 60 cycles for various maximum densities in the ring sample is shown in Fig. 1, in which Curve B gives the results for the annealed specimen, and Curve F for the unannealed.

* "A Complete Magnetic Testing Equipment," by J. W. Esterline. Vol. VI, 1906, page 320.

Epstein Method.—The Epstein apparatus is shown in Fig. 2, and consists of four coils, each about 12½ ins. long, and wound with

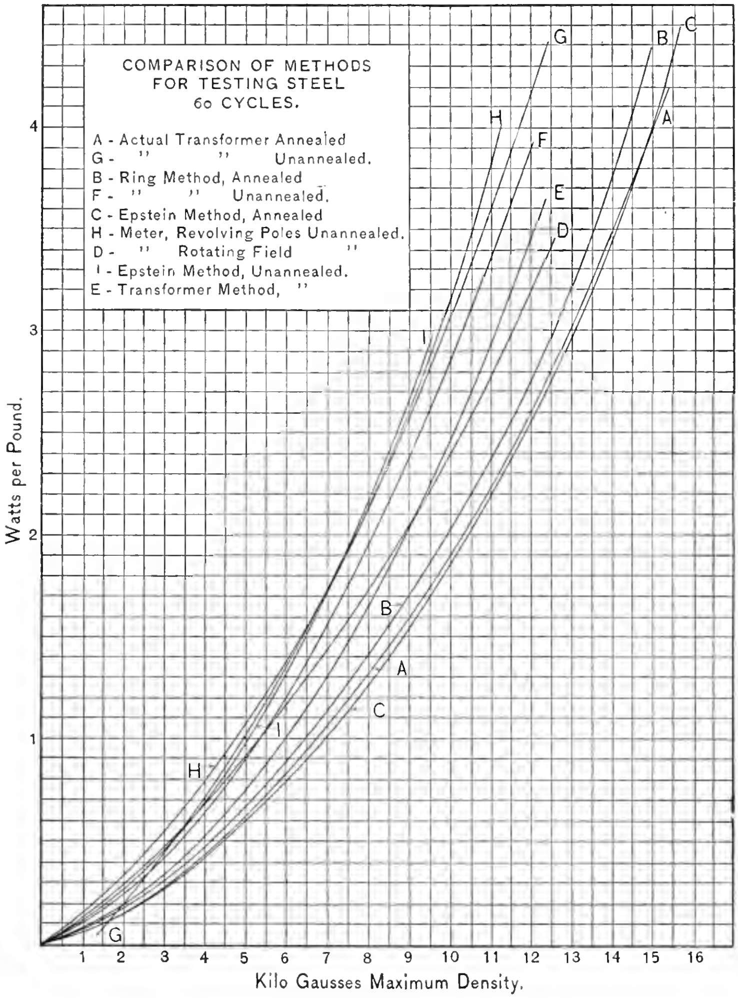

FIG. 1.

coarse wire on a bobbin, within which a specimen may be inserted for test and arranged in the form of a square, as shown in the figure. These coils are usually wound double, in order to reduce

the copper loss to a minimum, the two halves being placed in parallel. The four coils are connected in series to an alternating current source, giving a sine wave of electromotive force and having the desired frequency. The density in the specimen is determined from the equation of the transformer—thus

$$\text{Flux} = \frac{E \times 10^8}{4.44 \times F \times T}$$

where

F = frequency or cycles per second
E = E. M. F. in volts
T = the number of turns in series in the exciting coils.

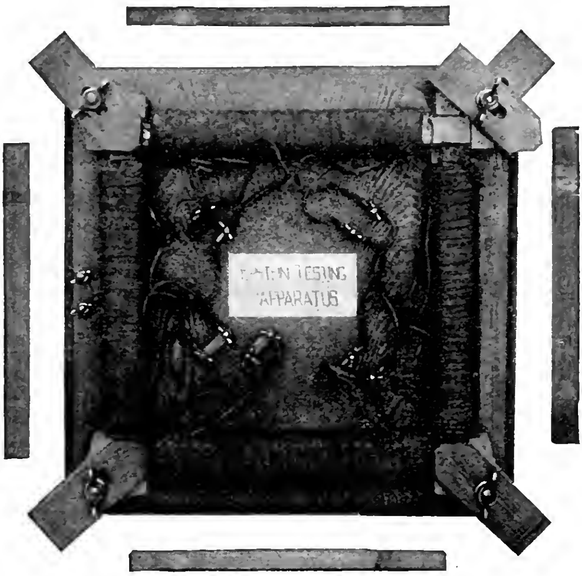

FIG. 2.—Epstein Apparatus.

Since the term flux involves density and area, a definite area of cross-section can be decided upon and constituted standard. For this standard cross-section a definite voltage is required, in order to give the magnetic induction desired. These constants are determined in the design of the apparatus, and must be closely followed to obtain good results. The core loss may be determined at any magnetic density by varying the impressed voltage. This

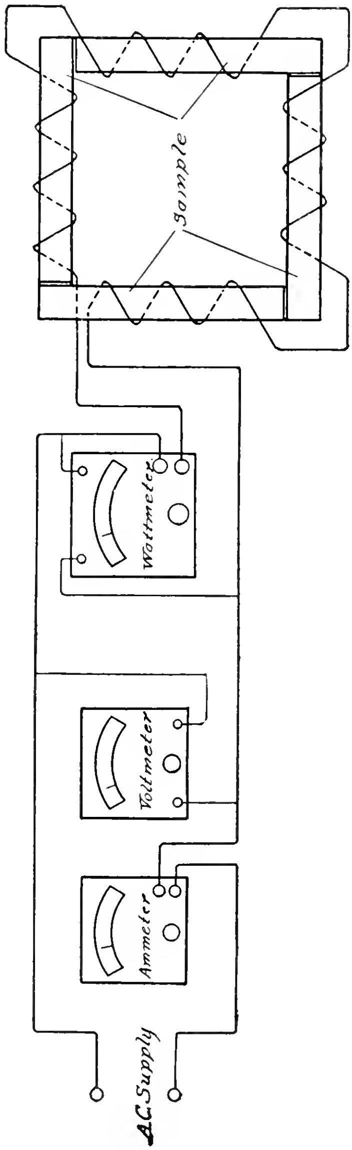

FIG. 3.

method is adapted to commercial use for the reason that the samples are easily made, are easily inserted, and but few readings are necessary to obtain reliable results.

The sample consists of about 15 lbs. of pieces, $15\frac{1}{2}$ ins. long by $1\frac{3}{16}$ ins. wide, cut in the direction of the grain of the sheet. This sample is divided into four equal sections, each held together by a string or friction tape. Butt joints are used at the corners, with a piece of thin tough paper inserted to prevent eddy losses at the ends. Clamps are provided at each corner for holding the sample. The specific gravity of the metal is usually taken at 7.77, and the ratio of net section to gross cross-section is usually taken at 0.9. This assumption gives 3.95 cu. ins. per lb., and the cross-section may be figured from this relation.

A wattmeter is used for determining the core loss; when this is connected as shown in Fig. 3, the copper loss in the magnetizing coils need not be corrected for unless the exciting current is excessive. The losses involved in the voltmeter and the pressure coil of the wattmeter should, however, be deducted from the wattmeter reading. The core loss resulting is divided by the weight of the sample, which gives the loss in watts per pound. In Fig. 1, Curve I gives the loss by the Epstein method for the unannealed sample, and Curve C for the annealed sample.

Holden-Esterline Core Loss Meter.—As previously described in the Proceedings of the Society, this apparatus consists of a field which may be revolved mechanically about a specimen, or which may be held stationary, and a rotating field produced by means of polyphase alternating currents. In these tests the samples were subjected to both the field flux revolved by mechanical means, and a rotating field such as is obtained in an induction motor. Curve H, Fig. 1, gives the losses in the sample when the poles were revolved mechanically, and Curve D the loss for the same sample when the alternating current rotating field was used. The sample was unannealed in both cases.

A standard core type transformer of small capacity was made of the samples provided for this purpose, and the losses measured by the wattmeter method. Curve A, Fig. 1, gives the results of the test of the completed transformer, built of annealed samples, while Curve G gives the results of the test of the transformer made from the unannealed specimens. Curve E is that given by

a method of testing used by a transformer manufacturer, in which the samples are laced up through a part of coils constituting a small core type transformer, without the core being clamped in position.

These tests bring out clearly the fact that seemingly accurate methods of making the core loss test may not agree closely, especially at the higher densities, but some of this lack of agreement may be due, in the tests made, to differences in the material itself, even though made from the same heat of steel. It seems, also to be well established that the Epstein method of testing gives results which compare closely with those obtained from completed transformers made from both annealed and unannealed stock. This is shown by the close agreement of Curves A and C for the annealed stock, and Curves G and I for the unannealed stock.

The results obtained by revolving the pole pieces mechanically also agree closely with those obtained by the Epstein method, while those produced by the rotating field fall uniformly below the other methods of testing on the ùnannealed samples. The ring method of testing does not seem to be in particular close agreement with any other method.

Further experiments along the line of comparing results of tests and the data from completed machines should enable the Committee to adopt a standard method of making core loss tests.

The Committee has conducted some investigations on magnet steel, which are still in progress.

Respectfully submitted on behalf of the Committee.

J. W. Esterline,
Chairman.

REPORT OF COMMITTEE H ON STANDARD TESTS FOR ROAD MATERIALS.

Although the Committee has held no special meetings during the year, much work has been done through correspondence. An extended investigation on the cementing value of road materials is now in progress. A new machine for testing this property has been constructed and in operation for several months, and has given excellent results. Your Committee hopes to present specifications for this test at the next annual meeting.

The hardness of road building rock has been under investigation for some time, and a test is now tentatively in use. Your Committee thinks, however, that the subject should be further investigated before the results are reported to the Society.

In 1904 your Committee reported specifications for an abrasion test for determining the resistance to wear of road building rock, and in 1905 specifications for a toughness test. As ample time has been given those interested in the subject to investigate these tests, and as no objection or criticism has been made to them, your Committee respectfully recommends that they be adopted by the Society.

Respectfully submitted on behalf of the Committee,

L. WALLER PAGE,
Chairman.

A. N. JOHNSON,
Secretary.

NOTE.—The Abrasion Test for Road Material, and the Toughness Test for Macadam Rock were adopted by letter-ballot of the Society on August 15, 1908, and follow this report.

AMERICAN SOCIETY FOR TESTING MATERIALS

PHILADELPHIA, PA., U. S. A.

AFFILIATED WITH THE

INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

STANDARD ABRASION TEST FOR ROAD MATERIAL.

ADOPTED AUGUST 15, 1908.

This well-known test is similar in almost all respects to the Deval abrasion test of the French School of Roads and Bridges. It has been used since 1878, and is entirely satisfactory for the purpose for which it was designed.

ABRASION TEST.

The machine shall consist of one or more hollow iron cylinders; closed at one end and furnished with a tightly fitting iron cover at the other; the cylinders to be 20 cm. in diameter and 34 cm. in depth, inside. These cylinders are to be mounted on a shaft at an angle of 30° with the axis of rotation of the shaft.

At least 30 lbs. of coarsely broken stone shall be available for a test. The rock to be tested shall be broken in pieces as nearly uniform in size as possible, and as nearly 50 pieces as possible shall constitute a test sample. The total weight of rock in a test shall be within 10 grams of 5 kilograms. All test pieces shall be washed and thoroughly dried before weighing. 10,000 revolutions, at the rate of between 30 and 33 to the minute, must constitute a test. Only the percentage of material worn off which will pass through a 0.16 cm. (1-16 inch) mesh sieve shall be considered

in determining the amount of wear. This may be expressed either as the per cent. of the 5 kilograms used in the test, or the French coefficient, which is in more general use, may be given; that is, coefficient of wear $= 20 \times \frac{20}{W} = \frac{400}{W}$. "W" is the weight in grams of the detritus under 0.16 cm. (1-16 inch) in size per kilogram of rock used.

AMERICAN SOCIETY FOR TESTING MATERIALS
PHILADELPHIA, PA., U. S. A.
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STANDARD TOUGHNESS TEST FOR MACADAM ROCK.

ADOPTED AUGUST 15, 1908

In the consideration of macadam road materials, toughness is understood to mean the power possessed by a material to resist fracture by impact.

In testing macadam rocks under impact, it has been found best to apply a number of blows of successively increasing energy and note the blow causing failure. The following test involving this principle is, therefore, recommended for determining the toughness of rock for macadam road building.

TOUGHNESS TEST.

1. Test pieces may be either cylinders or cubes, 25 mm. in diameter, and 25 mm. in height, cut perpendicular to the cleavage of the rock. Cylinders are recommended as they are cheaper and more easily made.

2. The testing machine shall consist of an anvil of 50 kgs. weight, and placed on a concrete foundation. The hammer shall be of 2 kgs. weight, and dropped upon an intervening plunger of 1 kg. weight, which rests on the test piece. The lower or bearing surface of this plunger shall be of spherical shape having a radius of 1 cm. This plunger shall be made of hardened steel, and pressed firmly upon the test piece by suitable springs. The test

piece shall be adjusted, so that the center of its upper surface is tangent to the spherical end of the plunger.

3. The test shall consist of a 1-cm. fall of the hammer for the first blow, and an increased fall of 1 cm. for each succeeding blow until failure of the test piece occurs. The number of blows necessary to destroy the test piece is used to represent the toughness, or the centimeter-grams of energy applied may be used.

REPORT OF COMMITTEE I ON REINFORCED CONCRETE.

The Committee on Reinforced Concrete desires to state that the Joint Committee with which it has been coöperating in the formulation of rules and regulations, covering the use of concrete and reinforced concrete, is now considering a report, but this report is not in such condition that it can be presented to the Society at this meeting. The report will, however, be presented at the next annual meeting of the Society.

The work of collating the existing literature and the results of previous investigations has been nearly completed, and it was the intention of your Committee to present this matter to the Society at this meeting. The character of the data thus collected is such that it must be very materially condensed in order to admit of publication. Your Committee has this under consideration and it hopes to arrive at some conclusion as to the best means of condensing it for publication.

The Joint Committee has been coöperating with the United States Geological Survey in the investigation of concrete and reinforced concrete and the results of these tests are being published. Already a number of bulletins have been issued and it is expected that there will be at least twelve available before the end of the present year. The bulletins thus far issued, some five in number, may be obtained without charge upon application to the Director of the United States Geological Survey.

Respectfully submitted on behalf of the Committee,

F. E. Turneaure,
Chairman.

Richard L. Humphrey,
Secretary.

REPORT OF COMMITTEE K ON STANDARD METHODS OF TESTING.

Committee K has held three meetings during the past year, one on December 27, 1907, one on January 28, 1908, and one on June 24, 1908. While considerable progress has been made in the work of several of the sub-committees, and of Committee K in connection therewith, the only action which they desire to report to the Society is the following resolution passed this morning by the Committee:

Resolved, That the report of the Sub-Committee on Metallography be accepted as a progress report; that it be presented to the Society for publication as a progress report for the purpose of inviting discussion, and that the Sub-Committee be instructed to continue its work with a view of preparing a final report.

The report of the Sub-Committee is appended hereto.

Respectfully submitted on behalf of the Committee,

GAETANO LANZA,
Chairman.

Appendix.

REPORT OF THE SUB-COMMITTEE ON METALLOGRAPHY.

The present state of our knowledge is such that we have only a general idea of the possibilities of the application of metallography. At present it is the technical expert who uses it most as a method of determining the causes of failure. He looks for bad material and studies the good for comparison.

Certain structures stand out as danger signals and their presence in certain material is a sign that it will be faulty. Such structures are, for example, in wrought iron, large areas of slag or cinder; in steel, even small areas of slag, etc., especially near the surface of the metal; comparative coarseness of grain in iron and steel; in cast steel, the coarse structure peculiar to under-annealed material; in annealed steel, the coarse structure characteristic of overheating, etc., and the segregation of the constituents ferrite and cementite. In cast iron, the size and form of the plates or flakes of graphite, the amount of combined carbon in the form of free cementite, etc., are of importance. To sum up the structure and state of aggregation of the constituents may be of far more practical importance than the actual chemical analysis, especially in those cases where we are liable to have segregation,

The polishing of the specimen is a well-known operation but the method of development of structure varies greatly. For unhardened iron and steel the following process has given satisfaction:

Microscopic Examination.—1. After polishing examine under a magnification of 50 to 150 diameters. Look for slag or cinder in wrought iron, manganese sulphide, etc., in steel,* and size and shape of graphite in cast iron.

2. Etch with a saturated solution of picric acid in alcohol for 15 seconds. This reveals the pearlite.† In wrought iron any

* Arnold and Westerhouse. *Jour. Iron and Steel Inst.*, 1903, I., 136. E. F. Law, *Jour. Iron and Steel Inst.*, 1907, II., 94.

† Igevsky, *Rev. de Mct.*, II. Lejeune, *Rev. de Mct.*, III., 426.

pearlite present shows up and the general appearance will sometimes show whether the material was puddled, etc., or made from reheated scrap. Several etchings are required to bring out the grain of the ferrite; this, however, can be done more easily and quickly by etching with 10 per cent. nitric acid, or 25 per cent. nitric acid in water; 4 per cent. nitric acid in amyl alcohol gives good results.

3. Near the entectoid point, viz., 0.6 to 1.0 per cent. carbon, it is often difficult to distinguish between thin envelopes of ferrite and cementite. This difficulty can be overcome by etching with a solution of sodium picrate, which turns cementite dark brown or black but does not color the other constituents . The solution is made by adding 2 per cent. picric acid to a 25 per cent. solution of caustic soda and is used at 100° C.*

The deductions to be drawn from the results of the above examination can come only from a comparison with standards.

For quenched and tempered material the results have not been so conclusive as in the case of softer steel, probably due to the fact that less work has been done on it. Coarse grain, segregation of constituents, presence of oxide and the like are all signs of bad material. For etching 4 per cent. nitric acid in amyl alcohol is used; the time will vary. Etch for 5 seconds and examine. Re-etch if necessary.‡

Macroscopic Examination.—This method of examination consists of cutting a section, filling it smooth and polishing with fine emery, after which the piece is deeply etched with some suitable reagent so as to show the structure. There is a difference of opinion regarding reagents for etching; dilute hydrochloric, nitric and sulphuric acids have been widely used. Iodine solutions, 10 per cent. ferric chloride and 10 per cent. copper ammonium chloride solutions have also been employed. A favorite solution in some works is

$$\left.\begin{array}{ll} H_2SO_4 \ldots\ldots\ldots\ldots & 200 \text{ cc.} \\ HCl \ldots\ldots\ldots\ldots & 100 \text{ cc.} \\ \text{Water} \ldots\ldots\ldots\ldots & 800 \text{ cc.} \end{array}\right\} \text{used warm.}$$

This method shows up defects due to segregation, blowholes,

* Kourbatoff, *Rev. de Met.*, III., 648.

‡ Kourbatoff, *Rev. de Mét.*, III., 648. Lejeune, *Rev. de Mét.*, III., 426. Heyn, *Mitt. aus dem Konig. Materialspruf. Gross-Lichter felde, West*, 1906, 29.

piping and the like, and when used in connection with microscopic examination yields valuable information.

In conclusion, it may be said that metallography as an adjunct to physical tests and chemical analysis is a very valuable aid in distinguishing bad from good material and explaining the difference.

Respectfully submitted,

W. Campbell,
Secretary.

REPORT OF COMMITTEE P ON FIREPROOFING MATERIALS.

Since the adoption of the "Standard Test for Fireproof Floor Construction," the attention of the Committee has been called to some desirable changes, and after careful consideration the following amendments to the "Standard Test" are submitted with the recommendation that they be referred to letter-ballot:

Insert after the paragraph reading "The floor may be tested as soon after construction," etc., a new paragraph reading, "No plastering shall be applied to the underside of the floor construction under test."

Alter the next following paragraph to read, "The floor shall be subjected for four hours to the continuous heat of a fire of an average temperature of not less than 1700° F.; the fuel used being either wood or gas, so introduced as to cause an even distribution of heat throughout the test structure."

To the end of the paragraph reading "At the end of the heat test a stream of water shall be directed against the underside of the floor, discharged through a one and one-eighth inch nozzle, under sixty pounds nozzle pressure, for ten minutes," add the clause, "the nozzle being held not more than three feet from the firing door during the application of the water."

The Committee has also taken up the consideration of a standard test for fireproof partitions, and as a result of its deliberations submits herewith a proposed standard test. Investigation along this line has not been so extensive as in the case of floors. Two well-established lines of investigation have, however, been made. That of the Bureau of Buildings, Borough of Manhattan, New York, included fire tests on forty-six types of construction. Of these seven types failed under the test; the other thirty-nine passed the test successfully. The British Fire Prevention Committee made a series of nineteen tests. We have no definite information as to how many of these were successful. Outside of these tests the only other investigation along similar lines discovered by the Committee is that conducted at the laboratory of the National Board of Fire Underwriters in Chicago, for and under the direction

of the Structural Materials Testing Laboratories of the United States Geological Survey. These tests were not, however, strictly speaking, on partition construction, but rather on materials. The areas under test were so very small compared with actual practical conditions that the Committee felt that the tests could not be considered satisfactory as partition tests.

In its recommendations the Committee has practically followed the requirements of New York City, except that they have increased the time of the test from one to two hours. In the opinion of the Committee this is a fairer, although severer, test. It also conforms more nearly with British practice. The test is intended to cover partition construction for what is known as "full protection." The Committee has under consideration a specification for "partial protection" to meet certain conditions in ordinary factory construction where the cost of "full protection" does not seem justifiable.

Proposed Standard Test for Fireproof Partition Construction.

The test structure may be located at any place convenient to the investigator, where all the necessary facilities for properly conducting the test are provided.

The test structure shall be of such design that the partition construction to be tested shall form at least one side of the structure. The other sides, roof, and foundations of the structure, may be of any materials and design that will withstand and confine the fire within the test structure for the required time.

At a height of not less than 2 ft. 6 ins., nor more than 3 ft., above the ground level, a metal grate, properly supported, shall be provided, covering the whole inside area of the building.

In the walls below the grate level, draught openings shall be provided, as many as possible, furnishing openings with an aggregate area of not less than one square foot for every ten square feet of grate surface. Means for temporarily closing these openings shall be provided.

Immediately above the grate level, in one of the end walls of the structure a firing door 3 ft. 6 ins. wide by 5 ft. high must be provided.

Flues shall be supplied at each of the corners, and more often

for a test structure with more than 250 square feet of grate surface, with sufficient opening to insure a proper draught. In no case shall a flue area be less than 180 square inches.

The size of the test structure will depend on the area of the partition construction to be tested. In no case shall the partition construction under test be less than 9 ft. 6 ins. high, nor less than 14 ft. 6 ins. long. This entire area must be above the level of the grate bars, and within such dimensions must not be reinforced or braced in any manner other than is done as an inherent and essential part of the system of construction. The edges may be supported in any manner fairly representing the conditions of support in good practice.

The width of the test structure at right angles to the partition under test shall not be less than 9 ft.

The construction to be tested shall be subjected for two hours to the continuous heat of a fire, rising in temperature to 1700° F. by the end of the first half hour, and maintained at an average temperature of 1700° F. for the balance of the test; the fuel used being either wood, gas or oil, so introduced as to cause an even distribution of the heat throughout the test structure.

The temperature obtained shall be measured by means of standard pyrometers under the direction of an experienced person. The type of pyrometer is immaterial so long as its accuracy is secured by proper standardization. The temperature should be measured near the center of the test structure about 6 ins. below the roof or ceiling and also at the center of each partition under test, about 7 ft. above the grate level. In case the partition under test is more than 15 ft. long, additional pyrometers shall be used, symmetrically disposed and not more than 12 ft. apart. Temperature readings at each point shall be taken every three minutes, and average used as the controlling temperature.

At the end of the heat test a stream of water shall be directed against the construction under test, discharged through a one-and-one-eighth-inch nozzle, under thirty pounds nozzle pressure, for two and one-half minutes, the nozzle being held within two feet of the firing door and the hose stream being played backward and forward over the entire surface of the partition under test.

The test shall not be regarded as successful unless the following conditions are met: No fire or smoke shall pass through the parti-

tion during the fire test; the partition must safely sustain the pressure of the hose stream; the partition must not warp or bulge, or disintegrate under the action of the fire or water, to such an extent as to be unsafe.

Respectfully submitted on behalf of the Committee,

IRA H. WOOLSON,
Chairman.

R. P. MILLER,
Secretary.

NOTE.—The amendments to the Standard Test for Fireproof Floor Construction embodied in the above report were adopted by letter-ballot of the Society on August 15, 1908. The Standard Test for Fireproof Floor Construction thus amended follows this report.

AMERICAN SOCIETY FOR TESTING MATERIALS

PHILADELPHIA, PA., U. S. A.

AFFILIATED WITH THE

INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

STANDARD TEST FOR FIRE-PROOF FLOOR CONSTRUCTION.

ADOPTED AUGUST 15, 1908.

The test structure may be located at any place convenient to the applicant, where all the necessary facilities for properly conducting the test are provided.

The test structure may be constructed of walls of any material not less than twelve inches thick, properly buttressed on all sides.

The floor construction to be tested shall form the roof of the test structure.

At a height of not less than 2 ft. 6 in., nor more than 3 ft. above the ground level, a metal grate, properly supported, shall be provided, covering the whole inside area of the building.

In the walls below this grate level, draught openings shall be provided, as many as possible, furnishing openings with an aggregate area of not less than one square foot for every ten square feet of grate surface. Means for temporarily closing these openings should be provided.

In the wall, immediately above the grate level, a firing door, 3 ft. 6 in. by 5 ft. high, must be provided in the side of the building at right angles to the floor beams. A second door must be added when the span of the floor slab under test exceeds ten feet.

Flues should be supplied at each of the corners, and oftener in case of a test structure exceeding 250 square feet of grate surface, with sufficient opening to insure a proper draught, securely supported and disposed at the sides of the structure in such

manner as not to rest on the floor under test. In no case should a flue area be less than 180 square inches.

The horizontal dimensions of the test structure will depend upon the number and the span of the systems under consideration. The clear span of the floor beams is to be 14 ft. The distance between floor beams, or span of slab, may be varied according to the design of the system to be tested, and should be as near as possible to usual practice. The underside of the construction under test must be not less than 9 ft. 6 in. nor more than 10 ft. above the grate level.

The construction to be tested should be designed for a working load of one hundred and fifty pounds per square foot, and no more. This load to be uniformly distributed without arching effect, and to be carried on the floor during the fire test.

The floor may be tested as soon after construction as desired, but within forty days. Artificial drying will be allowed if desired.

No plastering shall be applied to the underside of the floor construction under test.

The floor shall be subjected for four hours to the continuous heat of a fire of an average temperature of not less than 1700° F.; the fuel used being either wood or gas, so introduced as to cause an even distribution of heat throughout the test structure.

The heat obtained shall be measured by means of standard pyrometers, under the direction of an experienced person. The type of pyrometer is immaterial so long as its accuracy is secured by proper standardization. The heat should be measured at not less than two points when the main floor span is not more than ten feet, and one additional point when it exceeds ten feet. Temperature readings at each point are to be taken every three minutes. The heat determination shall be made at points directly beneath the floor so as to secure a fair average.

At the end of the heat test a stream of water shall be directed against the underside of the floor, discharged through a one and one-eighth inch nozzle, under sixty pounds nozzle pressure, for

ten minutes, the nozzle being held not more than three feet from the firing door during the application of the water.

After the floor has sufficiently cooled the load on the same shall be increased to six hundred pounds per square foot, uniformly distributed.

The test shall not be regarded as successful unless the following conditions are met: No fire or smoke shall pass through the floor during the fire test; the floor must safely sustain the loads prescribed; the permanent deflection must not exceed one-eighth inch for each foot of span in either slab or beam.

REPORT OF COMMITTEE Q ON STANDARD SPECIFICATIONS FOR THE GRADING OF STRUCTURAL TIMBER.

Meetings have been held in conjunction with the Committee of the American Railway Engineering and Maintenance of Way Association on Bridge and Trestle Timber, which Committee reported specifications for these timbers, for discussion, to the annual meeting of the Association in Chicago, in March. These specifications differed from those of this Society in several important particulars. Agreement is hoped for during the coming year. The Association adopted the list of defects and, with one exception, the trade names of species adopted by this Society.

The Committee has been active in bringing the specifications of the Society to the attention of organizations of lumber men. The Yellow Pine Manufacturers' Association has expressed its support of these specifications.

Progress has been made in the preparation of specifications for timber for building construction.

Respectfully submitted on behalf of the Committee,

HERMANN VON SCHRENK,
Chairman.

W. K. HATT,
Secretary.

REPORT OF COMMITTEE R ON UNIFORM SPECIFICATIONS FOR BOILERS.

In view of the fact that boilers, as objects of interstate commerce, are sent from any one state into any other, the necessity of uniform rules in boiler construction is apparent. The American Boiler Manufacturers' Association has since 1889 made many attempts through zealous members in several states, with the coöperation of its committee on uniformity, to have laws passed governing rational construction of boilers. In every case they failed through the opposition of some local interest. Their next move was in the direction of improving and unifying the boiler rules of the Steamboat Inspection Service, in the belief that the larger cities, situated on navigable waters, would naturally copy these rules and embody them in their ordinances, if they were made to conform to the best modern practice. Recognizing that the demands of an increasing interstate commerce would make uniform laws a necessity, they cherished the hope that under the interstate commerce clause, constitutional warrant might be found for congressional action. At the same time they gathered extensive information in regard to the best American boiler practice and embodied it in their "Uniform American Boiler Specifications," which represent persistent work on the subject from 1889 to 1905. Experience proved the great educational value of the rules therein laid down, by their adoption in many engineering specifications.

In 1907 the Legislature of the State of Massachusetts passed an act relative to the operation and inspection of steam boilers. Under this act a committee was appointed representing boiler using, manufacturing and insurance interests, and operating engineers; i. e. all the classes immediately dependent on good boiler practice. This "Board of Boiler Rules" was presided over by Mr. Joseph H. McNeill, Chief Inspector of the Boiler Inspection Department of the State of Massachusetts. The wide experience and intelligent study brought to bear upon the task by this board have resulted in a set of rules—published March 24, 1908—which cover every phase of the subject.

The adoption of these rules by a state always noted for the

high intelligence of its people, and one whose interests are mainly manufacturing, marks a decided advance in the movement for uniform boiler specifications. As one of the members of this Committee fairly puts it, "The rules are somewhat drastic in some respects, but they represent mainly the best engineering practice of the day."

The chairman of the Committee has had several meetings with the chairman of the Board of Boiler Rules, and takes pleasure in testifying to the fair, intelligent and rational methods which govern the actions of the Board. We believe that there is reason to hope that in recognition of the necessity of uniformity equally for engineering and commercial reasons these rules, modified in consequence of larger experience, will in due time be adopted by other states.

The interest of the Society in this matter lies in the question of materials. It is gratifying to report that the specifications for boiler steel are taken verbatim from the specifications of the Society. There is, however, ground for criticism in the chemical properties prescribed.

For flange or boiler steel the percentage of phosphorus is limited to 0.06 for acid and 0.04 for basic; for fire box steel 0.04 for acid and 0.03 for basic, and for extra soft steel 0.04 for acid and 0.04 for basic.

The question arises, what peculiar advantage has the acid over the basic process of making steel so that a higher percentage of phosphorus may be tolerated in the former than in the latter? We believe this distinction to be erroneous. It was probably adopted because it was known to be more difficult to reduce the percentage of phosphorus below a certain limit in the acid than in the basic furnace. There certainly is no proof that failures from excessive phosphorus would be less in acid than in basic steel. And as comparatively few steel plate manufacturers now use the acid process, it seems timely to change this by making the maximum limits equal for the two kinds of steel. Furthermore, we cannot see any reason for allowing a higher percentage of phosphorus in extra soft steel than in the fire box steel.

We believe that no steel plate should be allowed to go into a boiler which has more than 0.04 per cent. phosphorus. The percentage of sulphur is limited in the three kinds of boiler plate

mentioned, to 0.05, 0.04 and 0.04. These limits we consider too high. The chairman's experience shows that we cannot safely employ a boiler steel having more than 0.03 per cent. sulphur.

At most, the specifications of the American Boiler Manufacturers' Association, reached by agreement, after a long discussion by their committee and that of the plate manufacturers, should govern. These call for respectively, 0.04, 0.035 and 0.035 per cent. of sulphur. They were the result of a compromise, the boiler manufacturers very reluctantly conceding more than 0.03 per cent. This upper limit of 0.03 per cent, had governed in American Boiler Manufacturers' Association's specifications from 1889 until 1905, and there never was any difficulty in getting plate with such low sulphur content. In fact experience shows that fixing 0.03 per cent. as the upper limit resulted in steel having much less sulphur, by the very natural desire of first class steel plate manufacturers to remain well inside the danger limit.

A veteran inspector of most extensive and successful experience, writes us that crystallization has developed quite rapidly with boilers operated at the higher pressures demanded by modern steam practice. The Massachusetts rules attempt to meet this by abolishing lap joints and insisting on higher factors of safety. As a temporary expedient this is excellent. But if a material is poor or inappropriate, merely increasing its quantity is not a final or rational cure. Its quality must be improved. As steel makers met the requirements of the automobile builder by furnishing axles and braces of 120,000 lbs. per sq. in. tensile strength, they will certainly rise to the demand of the vastly more extensive steam boiler industry by eliminating the noxious metalloids and preventing their segregation. For uniformity in construction we must have uniformity in materials.

Experience in various and widely different kinds of manufacture warrants the belief that in all of them success depends more on carefully selected and tested materials than on manufacturing methods, machines or mechanical skill.

In discussing various demands of these Massachusetts rules, covering designs and methods more exacting and difficult than the average good shop practice of American boiler works, our criticisms were met by the experience of the inspectors as to lack of quality and uniformity in plate and tubes. Our own experience

coïncides with theirs. While undoubtedly our plate and tube mills can make better materials than ten years ago, the unprecedented demand for quantity has made quality uncertain. But on quality the continued success and growth of our manufactures will always rest.

The Massachusetts Board of Boiler Rules has reposed absolute confidence in our Society by adopting our specifications for materials. These rules will undoubtedly be widely quoted and in time adopted by other states and thus lead to uniformity. The responsibility resting on us as the originators of the steel specifications thus becomes increasingly grave. The Committee, therefore, deems it of the utmost importance that these specifications be revised and modified on the lines above indicated.

A minority report signed by two members of the Committee follows this report.

Respectfully submitted,

FRANCIS B. ALLEN,
R. C. CARPENTER,
HENRY J. HARTLEY,
J. E. SAGUE,
H. V. WILLE,
E. D. MEIER, *Chairman.*

MINORITY REPORT.

After careful consideration of the annual report of Committee R on Uniform Specifications for Boilers, we feel that we cannot sign the same for the following reasons:

In our judgment the report has a tendency to defeat the object of our Society in its attempt to accomplish a uniformity in specifications. We believe that the present standard specifications for open-hearth boiler plate and rivet steel presented by Committee A on Standard Specifications for Iron and Steel and adopted by the Society meet all the present needs of the boiler-makers and users. That they are favorably regarded is shown by their adoption by others interested in having good materials used for boiler purposes. The report of Committee R refers to the adoption of

these specifications for boiler materials by the Committee on Boiler Rules, appointed under the Act of 1907 of the Legislature of the State of Massachusetts.

Feeling as we do, and believing the specifications, if changed, should be changed by Committee A rather than by Committee R, we express our unwillingness to sign the Committee's report.

Respectfully,

C. L. HUSTON,
JOHN MCLEOD.

DISCUSSION.

Mr. Lewis.

MR. J. F. LEWIS.—The manufacture of .03 sulphur steel is a natural gas proposition. As our supply of natural gas diminishes and coal producer gas must be used instead, it means that he who insists on getting .03 sulphur steel will either not be able to get it, or that he will have to pay an abnormal price for it. I think it would be well to hold this question open, and get a little more discussion on it. Producer gas steel will run from .04 to .045 in sulphur. The .03 sulphur steel is practically out of the question with producer gas, but with natural gas we can make it.

Mr. Meier.

MR. E. D. MEIER.—I would like to say in regard to this, that it is not a new criticism. When in 1889 the first specifications were adopted by the American Boiler Manufacturers' Association this question was raised by several steel makers. The statement was made, "We could make it but at a high price." Mr. R. Hammond, one of our Committee, answered, "You make it and we will fix the price;" which was correct, because the demand and supply finally regulate the price. Then in 1897 I reviewed between 250 and 300 tests of material, representing eight different steel makers. Among the latter were some who had never been in the fortunate position of using natural gas, and there was not a single case of any sulphur going over .03. Afterwards, when this point was brought up by the Steel Plate Association, we tried to agree on a specification which would be the same for the boiler manufacturers and the steel-plate makers. We didn't want any impossibility, but we wanted steel that could be produced under normal conditions. I had then something like 580 tests of material, in addition to the 250 I spoke of, and as nearly as I can remember I am sorry I have not the exact figures here— there were only some five or six that reached .03 in sulphur. The discussion between the Steel Plate Association and the Boiler Manufacturers' Association ran over two years, and then finally at a joint Committee meeting we agreed, they coming down and we going up to .035 in sulphur. This compromise was agreed to by both parties with very good feeling, and this is all we are asking for now. I

Mr. Meier. will repeat what I said at the same time, that I would not put a plate of steel in my boilers in which the sulphur exceeds .03, because I believe even that too high, and I get steel now as before that has less than .03 sulphur.

A Member. A MEMBER.—We have been testing our boiler steel for the last three or four years and it invariably runs down to .025 in sulphur for acid steel.

Lately we have been getting some basic steel that has been giving about the same results, the sulphur running from .022 to .025. I see no reason why we should not get steel with as low sulphur as that. May I ask whether samples are to be taken from the ingot or the finished sheet?

Mr. Meier. MR. MEIER.—Generally we insist that it be taken from the large sheet.

A Member. A MEMBER.—Are these test bars to be taken from the bottom or the top of the plate? We have made some tests and we find that while a test from the bottom may show .025 sulphur, one from the top may give .045. We recently took out from locomotives two large plates that were cracked in the center, and an analysis showed similar results with reference to phosphorus.

Mr. Meier. MR. MEIER.—Mr. Charles L. Huston made some very interesting tests in rolling out several ingots into plates, dividing each into a number of sections, making physical and chemical tests from each and locating the segregation of metalloids. The results of his tests has made me more careful than before, for he showed that sulphur and other metalloids are found near the top in the center, in a sort of parabolic curve. So the safe plan is to discard more of the top of the ingot than would otherwise seem necessary.

A Member. A MEMBER.—In that case we ought to obtain the tensile strength at the top as well as the bottom of the sheet.

Mr. Meier. MR. MEIER.—The rules promulgated by the Steamboat Inspection Service call for tests from the two upper corners and the two lower corners of the plate as rolled. They first applied it to the sheets as cut out, which would make an interminable number of test pieces. I joined some of the steel men in arguing with them, and they agreed to take the tests from the plate as rolled.

REPORT OF COMMITTEE S ON WATERPROOFING MATERIALS.

Last year we announced in a general way our proposed program of action through the designation of two sub-committees, A and B, the former in charge of investigations of waterproofing materials other than those of a bituminous nature; the latter in charge of investigations of those of a bituminous character.

Sub-Committee A has carried on an extensive series of tests, both of materials incorporated with cement at the time of manufacture or added thereto or to the gauge water at the time of use, and of those materials used simply as protective coatings. The tests undertaken comprised the making of briquettes from cement mortars in the several proportions of 2 to 1, 3 to 1, and 4 to 1, of both a fair natural sand and standard quartz or Ottawa sand, with both treated and untreated material, to obtain comparative results concerning tensile strength at different periods.

Discs of identically the same material were similarly made up to determine the comparative effect on permeability of the treatment with the various waterproofing compounds. It has thus far been demonstrated pretty conclusively that, as was generally known, with the care and facilities of laboratory work, untreated mortars of a fair natural sand, even as lean as 4 to 1, can be made practically waterproof. Similar mortars of standard quartz or Ottawa sand failed to show corresponding impermeability when untreated, but when treated with several waterproofing materials showed considerable improvement in this feature, which, it can be concluded, was due to the mechanical improvement of the mortars by an increase in their granulometric value through the filling of the voids with the dry or, in case the compound is added as a liquid, suspended matter, rather than through any chemical action.

From the action of most of the materials experimented with it may be concluded that in a poorly proportioned mortar or concrete, and under the more unideal conditions in the field, where immediate results are wanted, many of the compounds examined do materially decrease the permeability of aggregates for the time being, but so far no claim for permanent action on their part is warranted. This condition arises in time with any well proportioned and properly laid concrete, through the mechanical filling of the voids by the percolation of water carrying natural deposits. An immediate condition of marked impermeability was, however, very generally obtained, with no impairment of strength, by the substitution of small percentages of very finely comminuted clay for sand, even when this was standard quartz or Ottawa sand. This was without the use of an electrolyte, which has been claimed to be helpful, although this was not confirmed by our experiments. No beneficial effect on tensile strength was noticed from the use of any of the so-called "waterproofing materials;" in fact, indications, only to be corroborated from much longer time tests, point rather to the impairment of strength, a weakening of the mortars, even though greater impermeability may be obtained.

The tabulation of the numerous results of tests is not given here, as these must be confirmed through the coming year before being published as conclusive.

Sub-Committee B has made little progress but much effort has been given to formulating a program of tests. A number of tests have lately been undertaken, the ultimate value of which has been somewhat questioned by the manufacturers of the materials to be examined. But these manufacturers have so far failed to make other suggestions of moment and it remains to be seen if the positive conclusions hoped for can be arrived at by the tests in question.

Sub-Committee A has purchased most of the material experimented with in the open market through a small fund provided by the Executive Committee.

The fact that Sub-Committee B has heretofore attempted to carry on its work without drawing on the fund will in part explain the delay on their part.

In conclusion, we feel that some progress has been made during the past year enabling, at least, Sub-Committee A to draw conclusions satisfactory to most of its members, but Committee S, as a whole, is not prepared to indorse these conclusions without further corroborative work, especially as the members of the Sub-Committee wish to be further fortified in their conclusions.

Respectfully submitted on behalf of the Committee,

W. A. Aiken,
Chairman.

A. W. Dow,
Secretary.

REPORT OF COMMITTEE T ON THE TEMPERING AND TESTING OF STEEL SPRINGS AND STANDARD SPECIFICATIONS FOR SPRING STEEL.

On behalf of Committee T, I wish to submit a report of progress. New problems have arisen within the year which have delayed the completion of our work. Several railroads have used one or the other of the specifications proposed a year ago, but have made no report.

Two features which have been brought before the Committee this year may be of present interest to the Society.

The first of these is a series of tests conducted by Mr. Converse, of the Baldwin Locomotive Works, which determined the effect of different methods of tempering on the elastic limit and the modulus of elasticity. The results of his tests are given in Table I.

Table I.—Results of Tests, showing the Effect of Different Methods of Tempering on the Elastic Limit and Modulus of Elasticity of Steel.

No.	Method of Tempering.	Elastic Limit. Lbs. per sq. in.	Modulus of Elasticity. Lbs. per sq. in.	
17.	Annealed in lead at 1,400° F.	78,500	27,550,000	
11.	Hardened in oil at 1,450° F. Drawn to 560° F.	137,500	28,700,000	
14	Hardened in oil at 1,450° F. Drawn to 560° F.	160,400	27,150,000	
19.	Hardened in oil at 1,450° F. Drawn to 400° F.	177,600	29,000,000	
12.	Hardened in oil at 1,450° F. Not drawn	187,400	28,610,000	
16.	Hardened in water at 1,425° F. Drawn to 1,050° F.	180,700	28,070,000	
13.	Hardened in water at 1,425° F. Drawn to 900° F.	233,900	28,860,000	
15.	Hardened in water at 1,425° F. Drawn to 750° F.	240,800	29,220,000	broke
20.	Hardened in water at 1,425° F. Drawn to 600° F.	219,800	30,420,000	broke
18.	Hardened in water at 1,425° F. Not drawn	212,000	29,960,000	broke

One interesting point clearly shown here is that the modulus of elasticity bears no close relation to the elastic limit. As the deflection under a given load bears a direct relation to the modulus, it is clear that the stiffness or deflection under a given load cannot be changed by hard or soft tempering. Likewise it is clear that the use of any alloy steels whatsoever, all of which tend to increase the elastic limit, cannot result in a material reduction in the number of plates or the weight of the spring, for although the elastic limit may be 50 or 100 per cent. greater than with the ordinary spring steel, duplicate springs will act alike under equal loads. On the other hand, the alloy steels with high elastic limits should give much longer service, since for the same load the ratio of the fiber stress to the elastic limit is materially less.

The difference in elastic limit, due to the difference in heat treatment, in the above tests brought up the question as to what fiber stress would be most satisfactory in springs. In transverse tests of soft-tempered spring plates it is found that at 375,000 to 400,000 lbs. per sq. in. fiber stress (the exact stress not having been determined), the plate fails without breaking, that is, it bends indefinitely under this load. The fiber stress at which this occurs, like the modulus of elasticity, seems to be independent of the elastic limit. It would seem reasonable to suppose, then, that the highest elastic limit that could be obtained and still permit the steel to bend to the point of failure without breaking, would give the best spring. On the other hand, a soft annealed iron wire will stand many more alternate bends beyond the elastic limit than will a hard wire. In order to determine if possible whether a low or high elastic limit is desirable, Massachusetts School of Technology has fitted up a machine which will permit of the application of repeated transverse loads. Duplicates of the Baldwin test pieces have been prepared, and these will be subjected to repeated loads of 140,000 lbs. per sq. in. fiber stress, releasing to 40,000 lbs. per sq. in., which approximates service conditions. These tests are being carried out with varying lengths, center to center, and varying thicknesses of steel. My laboratory is building a similar machine with the idea of carrying out similar tests on alloy steels and low carbon steel, such as is used abroad by Krupp and Steel Peach Tozer. This work is in progress and will be reported on next year.

The second point of interest is the investigation into the effect of banding, which was brought out by an investigation into the cause of the excessive failures of driving springs on one of our large railroads. On visiting the spring shop of this road the

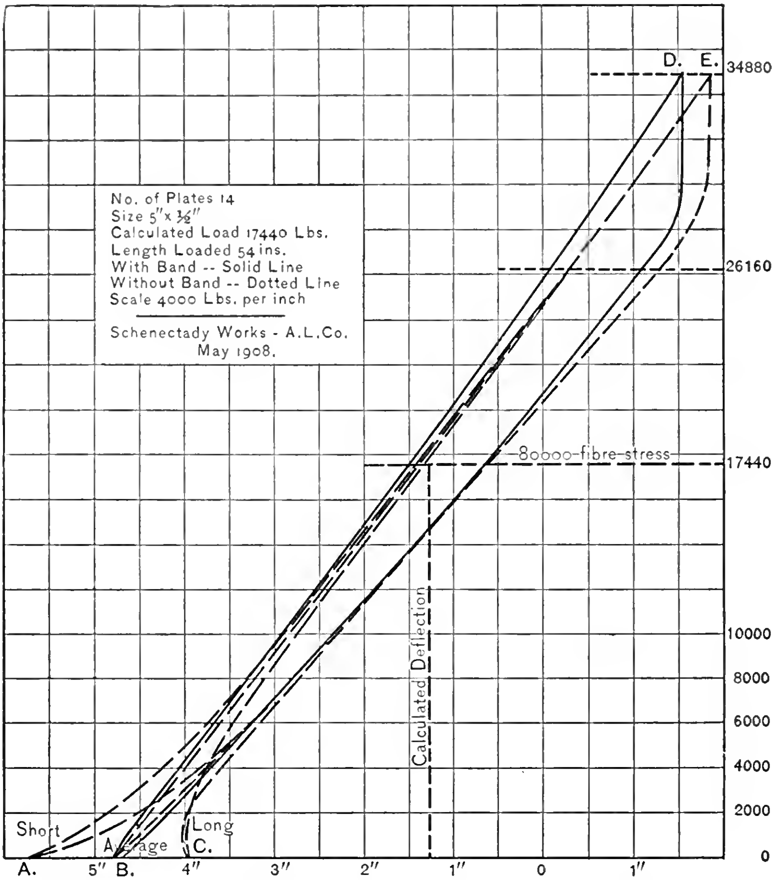


FIG. 1.—Test of Semi-Elliptic Spring with Band and without Band.

writer noticed the excessive difference in camber between the successive plates. It was at once apparent that when this excess camber was taken up in banding, a negative strain was put on the main plate, which would be given an extra camber, and an equal positive strain on the short plates which would be straight-

ened out. If the take-up was excessive or the spring very stiff, this strain could easily amount to an excess load of 30 per cent., or 24,000 lbs. per sq. in. fiber stress. The excess load the spring is always subject to in service should then result in heavy failures of the short plates. Railroad men who have investigated spring failures will recognize this condition. One road, on checking this condition, found an average of five failures of short plates to one of the main plates.

Fig. 1 shows the results of tests of a 14-plate spring, 54 ins. center, plates 5x½ ins., with and without the band. The numeric values appear in Table II. A, B, A is the actual height of top and short plate, tested without band after deducting the total thickness of the steel; A, E, C is the actual height of bottom or main plate without band; B, D, B is the actual height of spring with band. The short plates are drawn from A to B, or a total of 0.92 in. Unfortunately the actual change in height of the top

Table II.—Tests of Semi-Elliptic Spring, with Band and without Band.

Number of plates, 14; size, 5 x ½ ins.; span, 54 ins.

Load, lbs.	Height, ins.			
	Without Band.			With Band.
	Short Plate.	Long Plate.	Average.	
0	5.74	3.96	4.85	4.82
1,000	5.23	3.98	4.61	4.65
2,000	4.89	3.98	4.44	4.51
3,000	4.59	3.89	4.19	4.30
4,000	4.29	3.76	4.03	4.09
5,000	3.98	3.61	3.80	3.90
6,000	3.79	3.48	3.64	3.70
10,000	2.93	2.77	2.85	....
17,440	1.43	1.35	1.39	1.52
26,160	—0.34	—0.34	—0.34	—0.10
34,880	—1.85	—1.85	—1.85	—1.55
26,160	....	....	....	—1.14
17,440	0.59	0.59	0.59	0.63
10,000	2.34	2.34	2.34	....
6,000	3.34	3.24	3.29	3.23
5,000	3.59	3.45	3.52	3.44
4,000	3.87	3.62	3.75	3.72
3,000	4.17	3.81	3.99	3.92
2,000	4.55	3.96	4.25	4.17
1,000	4.99	4.03	4.51	4.42
0	5.66	3.96	4.81	4.71

plate was not observed, but it was undoubtedly equal to the effect of a load of 25 per cent., and the greatest deflection comes on the short plate, which is the least able to bear it.

One road had so much trouble with the short plates breaking that they resorted to the practice of putting in the short plate without tempering. I have so far been able to find only one specification which provides for the plates fitting close after the band is removed.

There has been considerable discussion at times as to whether, in calculating the deflection, the width of the band should not be deducted in order to obtain the effective length. In order to decide this by tests, it is necessary to construct a spring so that the plates fit perfectly. This was done with a 26-in. spring of 7,000 lbs. capacity. At 7,000 lbs. the deflection was the same for the spring with and without the band.

The difference in height of the 54-in. spring, with and without band, is due, I believe, to the increased friction produced by the band rather than to the shortening effect. At first glance it would appear that, in calculating the deflection, the length should be taken to the edge of the band instead of to the center; but when the spring, as usually constructed, is subjected to the slightest load, the main plates are free to act for their whole length, for when they straighten out they at once leave the band at the edges.

Respectfully submitted on behalf of the Committee,

J. A. KINKEAD,
Chairman.

DISCUSSION.

Mr. Kinkead.

MR. J. A. KINKEAD.—Mr. E. D. Nelson, of the Pennsylvania Railroad tempered some flat and round spring steel as follows: Some were hardened by heating to a temperature of 1350° F. and plunged in oil at 70° F. until cold, the temper being drawn in oil at 575° F. The duplicate bars were heated at 1350° F. and plunged in oil at 575°, then allowed to cool slowly in air to normal temperature. Mr. Macgregor, of Columbia University, was kind enough to make torsion tests of round bars and transverse tests of flat ones. I understand that Mr. Macgregor is prepared to report some data relative to these tests.

Mr. Macgregor.

MR. J. S. MACGREGOR.—I tested a number of steel bars in torsion which had been given various heat treatments under Mr. Kinkead's supervision, determining their elastic limit, ultimate strengths, and modulii of elasticity. It may be interesting to note some of the values that were obtained. For instance, the elastic limit in pounds per square inch for two sets of bars was, 46,000, 48,000, 43,000, and 41,000; and the ultimate torsional resistance in pounds per square inch was 113,000, 111,000, 117,000, and 117,000. In this series two of the bars received one treatment and two another. That is to say, we found the values 113,000 and 111,000 for bars receiving one heat treatment, and 117,000 and 117,000 for bars receiving another. The modulus of elasticity showed very little variation, and the value of G obtained for the above specimens was 12,300,000, 13,900,000, 13,500,000, and 12,030,000. It would be well to note that this value of G is approximately $\frac{2}{5}$ of the modulus of elasticity in tension. It would seem, therefore, that the modulus has very little variation with varying heat treatment, although there is a considerable variation in the elastic limit and ultimate strength. It may be of more or less interest to note that I could determine no set in these bars up to the elastic limit. However, immediately upon reaching the elastic limit in every case a definite set was obtained. I have treated some flat springs for transverse tests, heating them up to 1450° F. (past the critical point) quenching some in oil and others

Mr. Macgregor. in water, and then drawing them, some at 500° F. and others at 900° F. The strength tests upon these bars will be conducted in the near future. It is intended to take the deflection not only at the center of the bar but also to measure the compression of the end supports. In this way I hope to get a more accurate value of the transverse modulus and to overcome the difficulty of the compression of the end supports, which would otherwise doubtless affect the result. These remarks are simply intended to give an idea of the progress of the work.

REPORT OF COMMITTEE U ON THE CORROSION OF IRON AND STEEL.

Very considerable progress has been made during the past year in the study of the different phases of the subject delegated to your Committee. The evidence brought forward by various investigators in this field seems to be conclusive as to the fact that corrosion is an electro-chemical phenomenon. Since the discussion of the experimental work has been or will be presented and published in separate papers by the authors, it is considered unnecessary to take it up in this report. Much experimental work is still needed in order to determine whether or not the methods of preventing corrosion which are suggested by these researches will prove to be of practical value. It is satisfactory to note that a number of practical tests are already under way or are contemplated in the near future. It is the opinion of your committee, that progress may be expected along the lines expressed in the following classification:

Metallurgical Problems.—A.

(1) To determine if it is possible to manufacture steel or iron which shall be highly resistant to corrosion. This to include the influence of impurities, condition of segregation, heat treatment, etc.

(2) Study of methods of coating with other metals, such as zinc, copper, lead, nickel, etc.

Treatment Subsequent to Manufacture.—B.

(1) Study of effect of physical condition of surface, on resistance to corrosion.

(2) *Study of inhibitive and excluding coatings, such as pigments, vehicles, oils, varnishes, bitumens, and cement.

* B (2) should be studied in coöperation with Committee E.

Under Section A, coöperative tests have been planned between your Committee and the American Steel and Wire Company. The work will be begun immediately. Under authority of the President and Council of this Society, a sub-committee was appointed, consisting of A. S. Cushman, chairman, W. H. Walker and S. M. Rodgers, to draw up specifications for tests on steel wire. These specifications are as follows:

TEST No. 1.—EFFECT OF POSSIBLE SEGREGATION IN THE INGOT.

In a regular Bessemer heat in which the carbon is 0.10 per cent. or under, and the manganese between 0.5 and 0.8 per cent., two ingots are to be selected. One marked A, to be the first ingot poured from a heat, and one marked B, to be the last poured. These two ingots to be re-heated and rolled to billet size in the regular way. These pieces of metal to be cut into three sections marked A–1, which will represent the extreme top of the ingot, A–2 the middle of the ingot, and A–3 the extreme bottom of the ingot. The same selection is to be made from the sections from ingot B.

Each one of the above six sections is to be rolled and drawn, a portion of each section into No. 9 gauge wire and a portion into No. 12 wire. These two sizes of wire from each section are to be woven into separate panels about 300 ft. long, of regular style American fence, after galvanizing in the regular way.

TEST No. 2—INFLUENCE OF HIGH CARBON AND MANGANESE VERSUS LOW CARBON AND MANGANESE.

A quantity of extra B B wire with carbon 0.05 per cent. and manganese below 0.2 per cent. is to be drawn to the standard gauge and galvanized without wiping. A sufficient quantity of open hearth wire to be made with carbon between 0.50 per cent. and 0.70 per cent. and manganese between 0.50 per cent. and 0.70 per cent. is to be rolled and drawn to the same gauge and also galvanized without wiping. These are to be marked C–1 and C–2 and shall be long enough to mount at least eight strands of 300 ft.

TEST NO. 3—EFFECT OF INCREASING AMOUNTS OF MANGANESE ON BASIC OPEN HEARTH STEEL.

A number of heats of basic steel which shall not vary in any respect other than their content of manganese, is to be made and worked into standard gauge wire. This shall vary in manganese from the lowest percentage used in commercial steel to the highest manganese content practicable. The increase in manganese to be nearly as possible in increments of 0.1 per cent. These are to be marked D with successive numerals, and enough material should be provided to make standard eight-strand panels 300 ft. long.

In addition to, and independently of these tests, the opportunity will be given to manufacturers who control special methods for protecting steel to expose at the same time panels of wire fencing under the auspices of this Committee.

On completion of the wire panels called for under these specifications arrangement will be made for the exposure tests to begin probably in the city of Pittsburg or its neighborhood.

Under Section B tests are under way to test the power of certain inhibitive pigments for preventing corrosion. A number of steel columns under the steel pier at Atlantic City have been painted under the auspices of the Paint Manufacturers' Association of the United States, with inhibitive compounds, based on formulas supplied by the Chairman of this Committee. The formulas depend essentially upon the use of zinc chromate as the inhibitor. As the test is a severe one, the results should be watched with interest. Other tests of a similar nature are being planned for the ensuing year.

The need of a quick preliminary test for determining the resistance of steel and iron to corrosion is universally conceded. So far no better tests has been proposed than the standard acid bath which was tentatively suggested by your Committee at the last annual meeting. It is not yet possible to state definitely whether the results of the acid test are an indication of the rust-resisting power of the metal. A well-defined difference of opinion on this point must be recognized. It certainly does not seem right that observed differences in resistance to the acid test should furnish a basis for inferring that metal manufactured by one process is superior to that manufactured by another. A number of the

TABLE I.—IMMERSED IN DISTILLED WATER, MARCH 23, 1907.

Marks.	Area exposed in sq. in.	Loss in weight per sq. in., 6/12/07. 81 days.	Loss in weight per sq. in., 10/1/07. 191 days.	Loss in weight per sq. in., 5/20/08. 433 days.
S 2.......	9.41	.062 grams	.155 grams	.358 grams
S 3.......	8.70	.063 "	.149 "	.361 "
CI 2.......	9.11	.060 "	.144 "	.334 "
CI 3.......	9.11	.057 "	.142 "	.346 "
Z 2.......	9.73	.052 "	.128 "	.312 "
Z 3.......	9.73	.053 "	.132 "	.308 "

IMMERSED IN 3 PER CENT. SALT SOLUTION, MARCH 23, 1907.

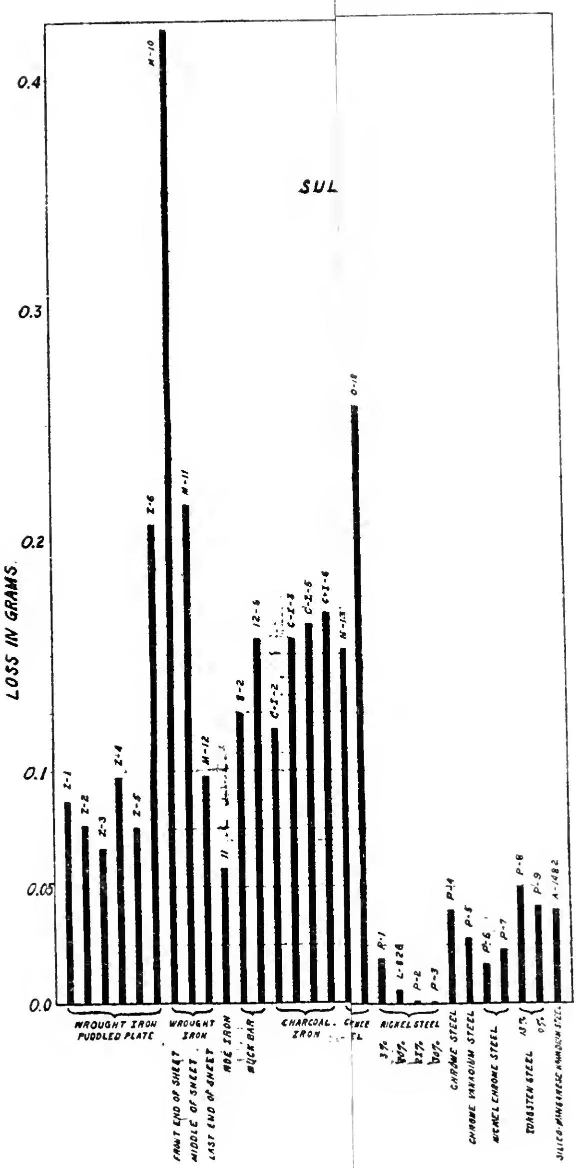
Marks.	Area exposed in sq. in.	Loss in weight per sq. in., 6/12/07. 81 days.	Loss in weight per sq. in., 10/1/07. 191 days.	Loss in weight per sq. in., 5/20/08. 433 days.
S 5.......	8.70	.077 grams	.231 grams	.544 grams
S 6.......	9.41	.093 "	.249 "	543 "
CI 5.......	9.11	.127 "	.269 "	.545 "
CI 6.......	9.11	.125 "	.255 "	.548 "
Z 5.......	9.73	.084 "	.221 "	.545 "
Z 6.......	9.73	.095 "	.241 "	.604 "

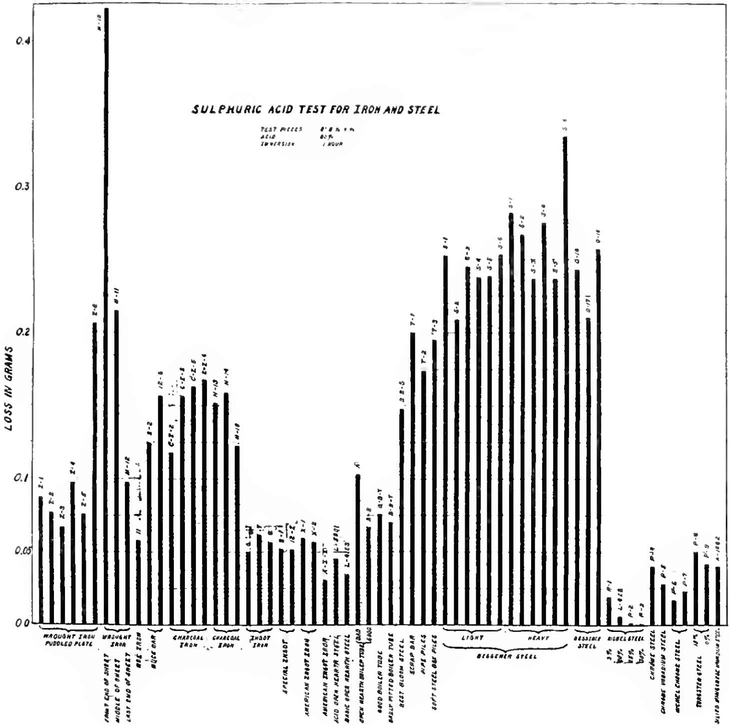
results of the test on samples supplied by members of the Committee have been plotted and are submitted to you herewith (Plate VII). Without drawing any definite distinctions, it may be said that there is a great variation in the resistance of samples of the same type of metal, probably due to segregation.

In the opinion of the Committee the tentative specifications for the acid test, as recommended last year, should be changed in one respect. The specifications read: "The results should be recorded as percentage loss, calculated on the original weight of the sample." This should be changed to read: "The results should be recorded as actual weight lost in grams or fractions thereof." It is recommended that the tentative specifications be reprinted in the next volume of the transactions and this correction inserted.

Since the life of some materials is limited by the localized action known as pitting it is the opinion of the Committee that too much dependence should not be placed on the acid test as indicating the relative life of the metal in service.

PLATE VII.
AM. SOC. TEST. MATS.
VOL. VIII.
T OF COMMITTEE U.




SULPHURIC ACID TEST FOR IRON AND STEEL
LOSS IN GRAMS
0.4
0.3
0.2
0.1
0.05
0.0
WROUGHT IRON PUDDLED PLATE
WROUGHT IRON
CHARCOAL IRON
INGOT IRON
BESSEMER STEEL
LIGHT
HEAVY
NICKEL STEEL
CHROME STEEL
TUNGSTEN STEEL
SCRAP BAR
PIPE PILES

Numerous experimenters have endeavored to determine the relative resistance to corrosion of iron and steel by immersing samples for varying lengths of time in water or salt solutions, the

TABLE II.—ANALYSES OF IRON AND STEEL.

Kind of Material.	Sample No.	Analyst.	S, per cent.	P, per cent.	Mn, per cent.	C, per cent.	Si, per cent.	Slag, per cent.
Wrought iron, puddled plate	Z. 1	National Tube Co.	.023	.120	.22	Tr.	......	......
		C. B. & Q. Ry. Co.	.028	.151	.19	.04	.207	.61
	Z. 2	National Tube Co.	.026	.134	Tr.	Tr.	......	......
	Z. 3	" " "	.033	.122	Tr.	Tr.	......	......
	Z. 4	National Tube Co.	.016	.118	.20	Tr.	......	......
		C. B. & Q. Ry. Co.	.017	.139	.11	.03	.174	.89
	Z. 5	National Tube Co.	.029	.126	Tr.	Tr.	......	......
	Z. 6	" " "	.020	.125	Tr.	Tr.	......	......
Charcoal, iron plates......	C. I. 1	National Tube Co.	.007	.056	Tr.	.07	......	......
		C. B. & Q. Ry. Co.	.007	.037	......	.04	.056	.58
	C. I. 2	National Tube Co.	.007	.040	Tr.	Tr.	......	......
	C. I. 3	" " "	.008	.033	Tr.	.07	......	......
	C. I. 4	National Tube Co.	.008	.032	Tr.	Tr.	......	......
		C. B. & Q. Ry. Co.	.008	.047	......	.02	.033	.74
	C. I. 5	National Tube Co.	.007	.041	Tr.	Tr.	......	......
	C. I. 6	" " "	.007	.034	Tr.	.09	......	......
Steel plates, Bessemer (light)	S. 1	National Tube Co.	.036	.109	.44	.10	......	......
		C. B. & Q. Ry. Co.	.032	.106	.45	.11	.038	.06
	S. 2	National Tube Co.	.052	.108	.42	.07	......	......
	S. 3	" " "	.030	.109	.34	.08	......	......
	S. 4	" " "	.040	.106	.37	.07	......	......
	S. 5	" " "	.036	.109	.47	.07	......	......
	S. 6	" " "	.048	.109	.48	.09	......	......
Steel plates, Bessemer (heavy)	S. 1	National Tube Co.	.034	.111	.44	.08	......	......
	S. 2	" " "	.034	.097	.43	.07	......	......
	S. 3	" " "	.051	.106	.44	.09	......	......
	S. 4	National Tube Co.	.028	.105	.47	.09	......	......
		C. B. & Q. Ry. Co.	.025	.103	.45	.09	.033	.11
	S. 5	National Tube Co.	.061	.096	.46	.08	......	......
	S. 6	" " "	.036	.115	.48	.10	......	......
Basic open-hearth sheet culverts, ingot iron....	8, 1	Am. Rolling Mill Co.	.022	.015	.10	.04	......	......
		National Tube Co.	.045	.039	Tr.	.09	......	......
		Am. Rolling Mill Co.	.028	.047	.09	.07	......	......
		C. B. & Q. Ry. Co.	.045	.046	.10	.11	.024	.04
			.043	.046	.11	.10	.021	.07
Ordinary steel culvert.....	8, 2	Am. Rolling Mill Co.	.049	.080	.42	.12	......	......
		National Tube Co.	.041	.097	.42	.07	......	......
		C. B. & Q. Ry. Co.	.042	.090	.45	.07	.032	.06
			.035	.071	.44	.07	.028	.08
Acid open-hearth steel	L. 5801	Lukens Iron & Steel Co.	.042	.028	.35	.17	......	......
Basic open-hearth steel	L. 4163	" " " "	.034	.024	.44	.18	......	......

amount of corrosion being determined by loss in weight. In order to see whether such tests as these are capable of giving any information in regard to resistance, the Committee collected a number of

TABLE III.—ANALYSES OF ALLOY STEEL.

Kind of Material.	Sample No.	S per cent.	P per cent.	Mn per cent.	C per cent.	Si per cent.	Cu per cent.	Ni per cent.	Cr per cent.	V per cent.	T per cent.
Nickel Steel.	P. 1	.032	.050	.600	.570	.239	...	3.34		...	...
	L. 820	...	...				...	20.00		...	...
	P. 2	.044	.031	1.55	.712	.548	...	24.05		...	...
	P. 3	.038	.028	1.27	.710	.470	...	20.65	...		...
Chrome Steel.	P. 4	.036	.038	.422	.382	.185	...	...	1.16	...	...
Chrome Vanadium Steel.	P. 5	.030	.014	.201	.353	.199	.016	Tr.	1.45	2.25	...
Nickel Chrome Steel.	P. 6	.034	.036	.370	.350	.190	...	3.00	1.10	...	...
	P. 7	.033	.035	.453	.385	.215	...	2.93	2.18	...	...
Tungsten Steel.	P. 8	.013	.035	.144	.655	.235	...	...	5.42	...	12.61
	P. 9	.010	.031	.141	.560	.226	...	...	2.41	...	10.25

typical samples of Bessemer steel, wrought iron, and charcoal iron. These were pieces of sheet metal, all prepared to a length of $5\frac{3}{4}$ ins. and a width of $\frac{7}{8}$ in. with about the original thickness of the metal, which varied from $\frac{3}{32}$ to $\frac{3}{16}$ in. The steel samples were marked "S," the charcoal irons "CI," and puddled wrought iron, "Z." In one of the tests which was carried to completion two pieces of each metal were suspended in distilled water, and two other pieces in 3 per cent. common salt solution. The samples were suspended so that a distance of about 1 in. remained above the surface of the liquid. The test was begun on March 23, 1907. The samples, twelve in all, were accurately weighed before immersion in the liquids, and the surface area of the part immersed was also determined. The samples were again accurately weighed on June 12, 1907. Another weighing was made on October 1, 1907. The final weights of the pieces were made May 29, 1908. The loss in weight per square inch was computed at each weighing, and these results, together with the total loss in weight, are recorded in Table I.

It is apparent that, as far as this test is concerned, the various samples showed only slight differences in resistance when compared with each other in the same liquid. In distilled water the various samples showed roughly about the same loss in weight per square inch, and the same is true in regard to loss of weight in the 3 per cent. salt solution, although, as would be expected, the loss

in the latter case was considerably larger. The analyses of all the samples shown in Plate VII are appended in Tables II and III.

It does not appear to your Committee that the results obtained from immersion tests of this nature are of definite value in determining the resistance to corrosion of various types of metal, unless the immersion bath is constantly aerated.

Respectfully submitted on behalf of the Committee.

Allerton S. Cushman,
Chairman.

Wm. H. Walker,
Secretary.

ELECTROLYSIS AND CORROSION.

By Allerton S. Cushman.

It is assumed for the purposes of this paper that by corrosion we mean the effects produced on the metals by the combined action of water and oxygen, with or without the stimulus provided by various impurities in the water or in the atmosphere. Few authorities now deny that in this sense of the word the corrosion of all metals is simply a matter of electrolysis. It is necessary, however, before proceeding with the discussion, to be quite sure that we are all accepting the same definition of electrolysis. The word was first used by Faraday to express the decomposition of compounds by the electric current, but to-day it is used in a wider sense, and electrolytic phenomena are recognized whenever a strip of any ordinary metal is immersed in water. There has unfortunately been a widespread impression that electrolysis, as it applies to the corrosion of metals, can take place only if the electricity, or electrical circuit involved, can be traced to some definite extraneous source. Thus it has come about that engineers, metallurgists and others to whom the causes of deep corrosion and pitting of metals is a matter of anxious inquiry, have wasted much valuable time, feeling about with voltmeters and galvanometers in vain efforts to locate and insulate the dangerous intruder. While it is not my present intention to combat the idea that stray, escaped currents play a contributory part in some cases of corrosion, I wish to emphatically point out that corrosion is eternally going on where no extraneous currents can be held responsible for the damage.

In order to understand exactly what is meant it will be necessary to consider briefly the modern physico-chemical explanation of electrolytic solution-tension. It is well known that when a liquid or a solid is heated, some of the molecules pass into the form of vapor or gas, and in any closed space equilibrium is established for a given temperature when the vapor exerts a certain definite pressure. As Nernst, who first gave expression to this phase of the modern theory of solutions puts it:

"If, in accordance with Van't Hoff's theory, we assume that

the molecules of a substance in solution exist under a definite pressure, we must ascribe to a dissolving substance in contact with a solvent, similarly a power of expansion, for here, also, the molecules are driven into a space in which they exist under a certain pressure. It is evident that every substance will pass into solution until the osmotic partial pressure of the molecules in the solution is equal to the solution tension of the substance."

To use a rough analogy, this is no more complicated than saying that a company of people pressing from one room into another will find themselves in comfortable equilibrium just as soon as the expansive power of the company in the newly-occupied space is about equal to that left in the originally overcrowded one. It should be understood, however, that individuals may be passing continually backwards and forwards between the rooms without essentially disturbing the equilibrium, but if we further assume that some barrier or force prevents a person having once passed into the new room from exerting back pressure, the stream of new arrivals will continue, to the depletion of the company in the original room. Also precisely the same result would accrue if the individuals pressing into the second room were slowly but constantly being removed by passing down a narrow stairway and thus being removed from the scene of action. Leaving now this rough analogy, whose application, however, will soon be apparent, we may return to Nernst's conception of solution-tension. The metals, probably without a single exception, have the possibility of passing into solution as positive ions, that is to say, as atoms carrying relatively to their mass enormous static charges of positive electricity . Every metal in water has a solution-tension peculiar to itself, provided it is pure, but as we shall see, this property is enormously modified under most circumstances by a most remarkably small presence of certain impurities. To quote from a well-known text-book on physical chemistry:*

"If we dip a metal into pure water, let us see what will take place. In consequence of the solution-tension of the metal, some ions will pass into solution. When metallic atoms pass over into ions they must secure positive electricity from some thing. They take it from the metal itself, which thus becomes negative. The

* "Elements of Physical Chemistry," Jones, p. 449

solution becomes positive, because of the positive ions that it has received. At the plane of contact of the metal and solution, there is formed the so-called electrical double layer, whose existence was much earlier recognized by Helmholtz.* The positively charged ions in the solution and the negatively charged metal attract one another and a difference of potential arises. The solution-tension of the metal tends to force more ions into solution, while the electro-static attraction of the double layer is in opposition to this. Equilibrium is established when the two forces are equal. Since the ions carry such enormous charges, the number that will pass into solution before equilibrium is established is so small that they can not be detected by any ordinary method."

It is apparent from this, that since, under the ordinary conditions of service, metals suffer corrosion only by first passing into solution, the corrosion can only be prevented or inhibited, either by aiding the resistance to the entrance of more ions into solution, or by covering the surface of the metal with a waterproof coating, or by doing both these things at the same time, on the principle that a double barrier is more impregnable than a single one. If we had to deal always with chemically pure 100 per cent. metals which were also perfectly physically constant as to surface condition, and if natural waters were absolutely pure, the electrical double layer would be sufficient to protect all metals from continuous corrosion. Needless to state, this is not the case. Let us then examine the forces which are operating in the direction of continued corrosion, as an understanding of these may point the way to various methods of throwing against them opposing forces. It is well known that even the purest water that it is possible to prepare is to some extent dissociated; that is to say, it contains free positively charged hydrogen ions, while ordinary natural water contains very many more free ions than pure water, as the measurement of its electric conductivity serves to show. Now, when this is the case the entering metallic ions do not have to take their positive charges with them, leaving the metal surface negative, but they now assume the positive charges carried by the hydrogen ions, which immediately pass into the atomic condition and make their exit from the system, as effectually as the individuals in our analogy did by proceeding

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* *Wied. Ann.*, 7, 337 (1879).

down the staircase. Relieved of the pressure of the hydrogen ions, further dissociation of the water takes place and the action may thus go on continuously, although in the case of most metals it is so exceedingly slow that it is not visible to the eye. The direct result of this action is that the electrical double layer is continually being broken down and if it breaks down at weak spots, which for any reason whatsoever exist on the surface of contact, we shall have corrosion taking place more rapidly at some points than at others and the results will be a pitting effect. Leaving this phase of the subject for the present, if we inquire which among the common metals have high and which low solution-tensions, we shall find that magnesium, zinc, aluminum, iron and the like, are always negative when in solutions of their own salts. This means that the solution-tension of the metal is always greater than the osmotic pressure of the metal ion in any solution that can be prepared. On the other hand, with such metals as gold, silver, mercury, and copper, the metal is usually positive when immersed in a fairly strong solution of its salt, although in very diluted solutions even these metals may appear negative.

Among the metals iron is unique in the fact that its physical characteristics are tremendously modified and changed by the presence of extremely minute quantities of other elements; thus, to mention one well-known instance, the electrical resistance of iron rises rapidly with an extremely small increase in the manganese content. That the surfaces of specimens of iron, when wetted, throw themselves into positive and negative nodes, corrosion taking place more rapidly at the positive poles, has been shown by means of the ferroxyl indicator, which was described in a previous paper.* It will be interesting now to consider and, if possible, to interpret the facts disclosed by this test, as well as the general subject of the rusting of iron, in the light of the modern theories of physical chemistry. When a strip of ordinary steel is wetted with pure water, iron ions under the pressure of solution-tension pass into solution, but, owing to the uneven electrical condition of the surface, local couples are formed, and the solution is stimulated at the positive poles while it is resisted at the negatives. If no oxygen is present, however, equilibrium is established by the

* *Bull.* 30, Office Public Roads, U. S. Dept. Agr.

formation of an electrical double layer before any visible damage is done. In other words, the positive iron ions tend to cluster or crowd about the point of their formation where they have inevitably left the metal slightly negative and the continued entrance of more iron into solution is thus resisted. Once admit molecular oxygen to the system, however, and the condition of affairs changes. As is well known, molecular or atmospheric oxygen has the power of oxidizing ferrous ions to the ferric condition, which results in the precipitation by hydrolysis of the insoluble hydrated ferric oxide which is called rust, and its consequent removal from the solution system. As ferric hydroxide tends to migrate to the negative pole the inevitable result is that the clustering ferrous ions are removed, one is tempted to say dragged, away from the entrance, the electrical double layer is destroyed and more and more iron ions stream in as time goes on, only to be in turn removed by oxygen. Thus the damage from pitting slowly but surely proceeds. The conclusion to jump at is that if we must admit water to the surface of iron or steel, it is dangerous to admit oxygen too. And at this point we find an explanation of some interesting facts that Professor W. H. Walker is at present studying, and to which he first called my attention; namely, that tannic acid and pyrogallic acid, when introduced into boiler water, prevent pitting. Since these organic compounds fix the dissolved oxygen in the water, there is but little left over to attack the ferrous ions.

Since oxygen undoubtedly plays such an important part in the corrosion of iron, how, it may be asked, are we to find an explanation of the now widely known fact that such strong oxidizing agents as chromic acid and its salts prevent the corrosion of iron? The paradox seems the greater when we proceed to the consideration of this phase of the subject directly after having pointed out that a strong reducing agent like pyrogallic acid produces the same result. In order to understand the subject better, we must review what is known about the so-called passive state into which certain metals may be put, and here again I shall quote from a recent work on physical chemistry:*

"It has long been known that when certain of the metals are subjected to special kinds of treatment, they no longer have the

* Jones, *loc. cit.*, p. 440.

properties that they usually possess. As early as 1790 Kier* observed that when iron is dipped in nitric acid having a specific gravity of 1.45, it becomes passive, i. e., it is no longer attacked by dilute nitric acid. Further, it no longer has the power to precipitate metallic copper from a solution of a copper salt.

"Other strong oxidizing agents, such as chromic acid, also render the iron passive. The same result is frequently obtained when iron is made the anode in electrolysis.

"A number of metals other than iron can be rendered passive. We should mention especially chromium, copper, cobalt, and nickel.

"A number of attempts have been made to explain the passivity of the metals. Faraday† and Schönbein explained the passivity in the case of iron, as due to the formation of a layer of oxide on the surface of the metal. This was natural when we consider that iron is rendered passive by strong oxidizing agents, and loses its passivity when heated in a reducing gas.

"The oxide layer theory of passivity is now regarded as untenable, since the passive state has been brought about under conditions where oxidation is impossible; and, further, has been destroyed under conditions where any layer of oxide if formed would not be disturbed.

"The same fate has befallen the theory that passivity is due to the formation of a protective layer of gas over the surface of the metal. The two views of passivity that have acquired the greatest prominence are those of Finkelstein‡ and Hittorf.§ According to the former, active iron is bivalent and passive iron trivalent. This conclusion was based upon the difference in potential between iron electrodes and the iron salt in which they were immersed. The potential difference depends upon whether the iron salt is in the ferrous or in the ferric condition.

"Hittorf also points out that in the case of chromium, the passive condition corresponds to the highest valence, and the active to the lower valence. He thinks that we have to do with two allotropic modifications of the elements, one of which is active and the other not."

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* *Phil. Trans.* 80, 350 (1790).
† *Phil. Mag.* (3) 9, 53 (1836); 10, 175 (1837).
‡ *Ztschr. phys. Chem.* 39, 91 (1901).
§ *Ibid.* 30, 481 (1899); 34, 385 (1900).

If Finkelstein and Hittorf are correct, and the experimental work appears to justify their conclusions, the action of chromic acid and its salts in preventing the corrosion when present in sufficient concentration is very simply explained. Passive trivalent iron does not pass into solution and ionise and the reaction which produces oxidation can not proceed. The second theory, however, which holds that the protection afforded by chromic acid is due to a polarization effect, and that the metallic surface is actually plated with ionic oxygen, is still very widely held in spite of the opinion advanced in the above quotation. Moreover, the polarization theory explains other effects which have been observed in the course of recent investigations along these lines, carried on independently by W. H. Walker and by myself. In my own laboratory it was observed that in making ferroxyl mounts an indentation, scratch, or wound of any kind on the surface of the steel invariably became positive to its surrounding area, and thus formed a center of corrosion. If this had occurred only upon mill specimens, which carried a coat of scale or blue oxide, the explanation would be simple, but the fact is, if a freshly polished steel mirror receives a cut or wound before immersion, the marked place comes out in blue and corrosion naturally takes place more rapidly at the positive spots.

The fact thus brought out by experimental investigation has a direct bearing upon the results of observation in practice. I am informed by Commander Parks, of the United States Navy Department, that it has been known for a long time that indentations or injuries on the water surfaces of boilers always become centers of corrosion and pitting. It is certain that inspection should be very thorough in regard to this point, where resistance to corrosion is a matter of prime importance. It is a question whether the smoothing up and actual polishing of surfaces would not in the long run repay the trouble and expense. There are times when a sound boiler tube is of vastly more importance than a polished gun barrel.

It would seem from the facts brought out by the ferroxyl test that the polarization effects which take place on the surfaces of iron or steel are determined by a number of widely different causes which may be classified as electrical, chemical and mechanical. It seems probable that the surfaces of steel which are subject to the

condensation moisture of the atmosphere are always in a certain state of electrical strain and polarity. Any change produced on the surface by cutting, stressing or straining upsets the equilibrium, with the result that certain surface points are depolarized. The ferroxyl tests show that in the very large majority of cases the positive spots once formed remain positive, and so corrosion proceeds steadily to the formation of destructive pit holes.

As has been pointed out in previous papers, the purer and more homogeneous, chemically speaking, the iron or steel, the less liable is the metal to suffer from localized rusting leading to pitting; but, besides the important bearing which this chemical homogeneity has upon the problem, we now see that the physical and mechanical condition of the surface is of the greatest importance. It is undoubtedly true, as Burgess and Walker have shown, that the rapidity of corrosion is modified by the condition of strain that the metal may be under, and this, as has been pointed out, is undoubtedly due to the changes set up in the electrical state or condition of polarity on the surface of the metal. Again, the presence and distribution of mill scale on the surface is a modifying factor, as has been shown by Walker in an exceedingly interesting experiment.

All these factors must be taken into consideration when attacking this interesting and intricate subject, but I should wish to urge upon metallurgists the prime importance of studying the effects of the usual metallic impurities which accompany nearly all forms of merchantable iron and steel. Already many metallurgists are agreeing that the use of manganese in steel making has been overdone, and they are seeking to reduce the manganese content to the lowest practicable quantity. It was new to me until a few months ago, and I think must have been overlooked by many other workers along these lines, that impure metallic manganese, when in a fairly finely divided condition is soluble in water, decomposing it with a rapid evolution of hydrogen, as I can easily show you. Since this is the case it is not surprising, if even slight segregation has taken place in manganiferous steel, that the polarization effects would be modified and increased.

Another interesting experiment shows that certain metals can be so mixed as to give off a very rapid evolution of hydrogen when brought in contact with cold water. I have studied the action of water on a number of combinations of metals by mixing them

together in the form of powders and then compressing the mixtures in a die into tablets under a pressure of 50,000 lbs. per sq. in. The amount of hydrogen given off and collected in a given time by equal weights of these compressed tablets when acted on by cold water is distinctly modified and determined by the electrolytic effects which are produced. Metallic magnesium is stated in the text-books on the authority of Liebig, Bunsen, and others, not to decompose cold water, but we have not as yet encountered a sample of magnesium that did not vigorously give off bubbles of hydrogen in cold water. An interesting experiment can be shown in which tablets made from pure aluminum and pure zinc are placed in cold water side by side with a compressed alloy of the two. The first two tablets are not visibly attacked by the water while the hydrogen comes from the alloy with all the vigor exhibited by an effervescent salt.

It is safe to state that the tendency of metallic impurities to produce electrolytic effects on the active surfaces of the various industrial metals, has not received the attention that the problem demands.

As I stated last year there are two distinct lines along which progress may be made in combating the damage done by corrosion. One is a metallurgical and the other a paint problem. The first I have already to some extent treated, and on the second I shall have something to say later. It is sufficient to state here that the evident palliative measure of applying a first or prime coat containing a substance which prevents corrosion by inhibiting electrolysis gives great promise for future experimental work.

It has been frequently shown that alkalies inhibit corrosion while acids stimulate it, for reasons which have been discussed by Professor Walker and myself in recent publications. It would be well worth trying whether trenching with lime would not furnish protection to steel pipe lines, that have been reported as giving trouble.

Although the electrolytic theory of the corrosion of the metals may not have met with universal acceptance, it is difficult to see how it can be rejected in view of the evidence that has been brought forward by recent workers in this field. But whether it survives or not, like other good theories, it will serve a purpose in suggesting methods of overcoming existing difficulties.

THE RELATIVE CORROSION OF STEEL AND WROUGHT IRON TUBING.

By Henry M. Howe and Bradley Stoughton.

Is steel, tube steel, intrinsically and incurably materially more corrodible than wrought iron, as unprotected steel and iron are surely far more corrodible than well-painted steel and iron? Or is it merely that ill-made steel, and steel of unsuitable composition, are more corrodible than well-made wrought iron? If the former, then in each and every test which is sufficiently wide to make reasonable allowance for the usual caprices of corrosion, tube steel must necessarily corrode and pit materially more than wrought iron. If the latter, then, though in certain tests tube steel may corrode and pit more than wrought iron, either because that steel is ill-made or of unsuitable composition, or because of the caprices of corrosion, yet in other tests tube steel should resist corrosion as well or better than wrought iron.

If the latter proves true, then has such a degree of skill in manufacture and inspection been reached that, in each lot of one hundred or one thousand or ten thousand steel tubes delivered, there need be no single tube which shall corrode or pit materially more than the worst tube in a like lot of wrought iron tubes? Is steel as trustworthy as iron?

To these questions this paper seeks an answer from the evidence at hand, in view of the existing distrust of steel, indeed the general belief that steel is intrinsically and incurably far more corrodible than wrought iron. Where we touch on other questions, we do it to throw light on these.

The day has gone by when the Society can hear with patience that, because some steel of unknown source has misbehaved, therefore steel cannot be so made as to behave well. The serious study and great efforts made in the last decade to fit steel tubing to resist corrosion are not to be ignored. To ridicule them would be ridiculous. Have they or have they not yielded regularly a steel which resists corrosion substantially as well as wrought iron?

What we have to say is based in part on investigations which

we have made on behalf of the National Tube Company, and in part on our independent inquiries along lines which suggested themselves to us while we were making those investigations. As we understand, that company is interested in overcoming what it believes to be the existing prejudice against steel tubes.

Our inquiry relates only to uncoated tubes. It does not concern itself with the relative merits of steel and iron for conduits, the life of which depends upon the integrity of their coating.

There is no reason why steel ought to corrode worse than wrought iron, at least no reason strong enough to call for unusually convincing evidence. The most marked constant difference between them, the presence of cinder in iron and its absence from steel, creates no such reason. In that the particles of cinder themselves resist corrosion they protect the metal beneath. But their distribution is such that this protective effect may be equaled or even outweighed by their opposite effect of hastening corrosion by difference of potential. This distribution is shown in the accompanying micrograph (Fig. 1). To increase this mechanical protection by increasing the quantity of cinder should hardly be practicable, at least in case of tubes which need strength, because this would weaken the metal tangentially, i. e. transversely. In cases in which strength may profitably be sacrificed to gain incorrodibility, it may perhaps be practicable to make use of this principle. This might perhaps apply to the metal for certain tanks.

The evidence, which is presented and discussed in detail in an appendix, may be summarized as follows:

Steel corrodes and pits less than wrought iron in our own tests (A) lasting seven months, on twelve pieces of steel skelp in competition with ten pieces of wrought iron skelp from the best makers, in hot aerated salt water, a medium previously found extremely unfavorable to steel; (D) in Principal T. H. Thomson's tests on three steel and three iron tubes for about a year in hot water under service conditions; (E) and (H) in simultaneous exposure of many steel and iron pipes to sulphuric acid coal mine water; (J) in the actual use of eleven steel and eight iron tubes in railroad interlocking and signal service; (K) in certain locomotive boiler service; and (N) in tests in which sixteen pieces of wrought iron and steel tubing were buried in dampened ashes

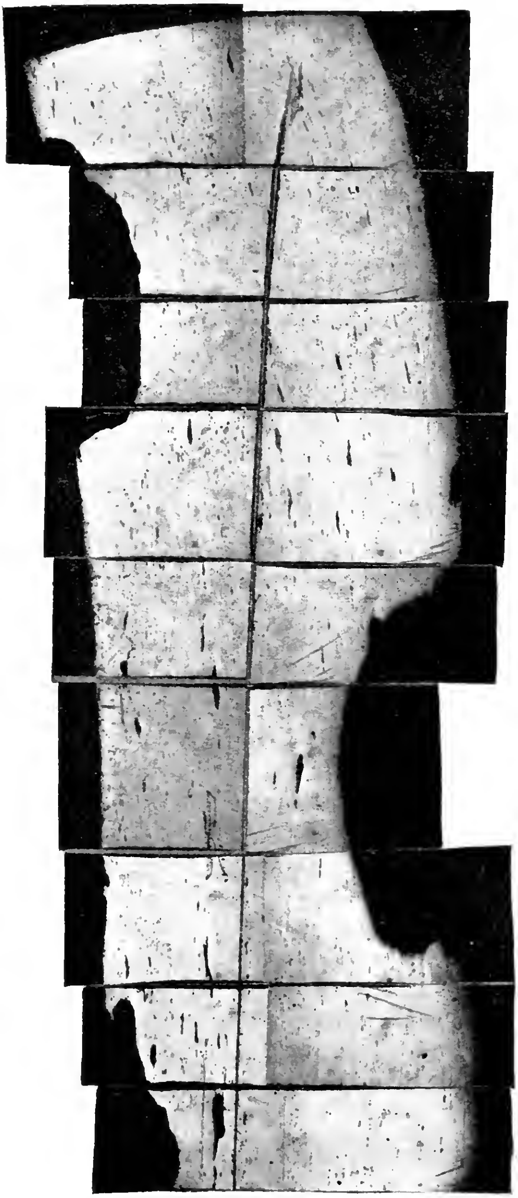

FIG. 1.—Charcoal Iron Boiler Tube, Transverse Section of Three Pits. Magnification, 17 diameters.

for sixteen months. In cases E, H, J and K the tests were carried to destruction.

Cases KK and L, trials in locomotive and in stationary boiler tubing, tended to show that there was no material difference between steel and iron. Case O, a twenty-six-month test in the Gayley blast drying coil at the Isabella furnaces, showed no difference between steel and iron, both of which had scaled uniformly.

Five cases, C, F, G, I and Q, are more or less unfavorable to steel Of these C and Q relate not to modern steel, but to that of 1897 or earlier, and Q, indeed, reports a condition of affairs wholly exceptional; while the evidence under I is obscure if not self-contradictory, and is not shown to apply to modern steel tubing. In case F, the only one of those unfavorable to steel which is known to apply to modern steel tubing, on simultaneous exposure, pushed to destruction, to sulphuric acid mine water, the average life of three steel tubes was 11 per cent. less than that of three iron ones in series with them, and the life of the shortest-lived steel tube was 14 per cent. less than that of the shortest-lived iron tube.

This is all the evidence which we have found, and received permission to cite, though we have asked manufacturers prominently and financially interested in showing that steel is worse than iron to give the addresses of those who could give us evidence. None of that which we have found, but have not yet received permission to cite, is unfavorable to steel.

To sum this up, tube steel has corroded less than wrought iron in seven distinct sets of tests, by seven different sets of observers in seven different places. In three other sets steel and iron behaved substantially alike. Eight of these ten sets were under conditions of service, and in six of them corrosion was pushed to destruction. In five cases steel corroded worse than wrought iron, but in the only one of these in which the steel tube is known to be modern the difference was moderate. Further, in our own tests (B) steel tubing of 1906 pits very much less than that of 1897 from the same makers.

The fact that steel has behaved as well and often better than wrought iron in so large a number of tests, seems to us cogent evidence that steel is not intrinsically materially more corrodible

than wrought iron. The fact that in one set, F, modern tube steel has corroded a little worse than wrought iron does not conflict with this inference in the least.

Opposed to this evidence there is a very widespread and deep distrust of steel tubing, indeed a belief that it habitually pits deeply, and that wrought iron corrodes uniformly, a belief contradicted by the evidence under V. This distrust, so far as we know, is not based on any direct competitive tests between materials known to be good modern steel and wrought iron respectively; note that we restrict ourselves to known good modern tube steel, made specially to resist corrosion Instead, this distrust seems to rest on the results of practical experience.

Most of this experience cannot have been with good modern steel tubing, but must have been with the older tubing which preceded it. Now, the present survival of this distrust in spite of the apparent great improvements in making tube steel resistant, and in spite of the evidence that it does resist as well as iron, need not surprise us in the least. Such a survival is always easy. It is especially easy under the unusual conditions of this case, viz., that the user has not been able to tell steel from iron by his own observation, and that in a large proportion of cases tubing sold as wrought iron has actually been steel, as shown under J and T in the Appendix. We believe that we are right in saying that, of the three great classes of users, architects, civil engineers, and plumbers, the last only have in general attempted to learn by direct personal observation whether a given tube was steel or iron. This the plumber has thought that he could do with certainty by the threading test; but the evidence under S shows that this test, at least in most hands, is useless. It is beside the mark to say that he could have distinguished steel from iron by the etching test, because, as we understand, this test has not in fact been used to any important extent With the best intentions, steel and iron carried in stock are likely to get mixed up, unless special precautions are taken to keep them separate. The fact that neither dealer nor user could tell them apart has removed one usual motive for preventing errors.

This actual ignorance of users as to whether any given lot of pipe is steel or iron seems to us of value, not of course in showing whether steel or iron is the better, but in interpreting the existing

distrust of tube steel in the face of direct cogent evidence tending strongly to show that this distrust is unjust.

The truth must fit all the facts, and no fact can be rejected simply because it does not fit our preconceived belief as to what the truth is. The truth is what the facts, all the facts, show, and not what we may in the past have thought they showed. No matter how strong we may think the presumption created against tube steel by the very existence of the present distrust, no matter how heavy the burden of proof that we may demand of the defenders of steel, we have no right to shut our eyes to the evidence. Nobody has a right to say either that defects which may in the past have lessened tube steel's resistance cannot be cured by skill and care, or, except on valid evidence, that they have not been cured. The existence of distrust calls for caution, but does not justify incredulity, especially in view of the special conditions of this case.

The theory that steel is intrinsically and incurably more corrodible than wrought iron is contradicted flatly by the evidence above. It therefore must be abandoned, unless resuscitated by correspondingly direct, convincing, and abundant counter-evidence, clearly relating to well-made modern tube steel. Nobody doubts that ill-made steel may misbehave. The distrust of steel may be nothing but a survival from a day when it was justified, or it may be only an unjustified inference that, because some steel corrodes badly, all must. The distrust can be explained away; the evidence cannot, Therefore the evidence and its implications must stand till refuted.

Our second question now arises, "Have manufacture and inspection been so perfected that, reproducing continuously the conditions which have made so much of the steel of our present evidence as incorrodible as wrought iron, they may deliver only steel of like incorrodibility?" That they will be so perfected some day we can hardly doubt; but have they been already? To prove this so that the community can rely confidently on it, needs more evidence, especially of cases in which corrosion is pushed to destruction. We understand that such evidence is accumulating rapidly. But though not proved, it is made easily credible, indeed to our minds on the whole probable, (1) by the very fair degree of harmony among the large number of tests of

modern steel reported; and (2) by the fact that out of the nineteen pieces of modern steel tubing represented in cases A, D, and E, not a single one shows any abnormal pitting, nor behaves materially worse than the worst of its iron competitors, of which those in test A at least were from the best makers. This probability is somewhat strengthened by the many cases summarized under P.

Appendix.

DETAILED EVIDENCE AS TO RELATIVE CORROSION OF STEEL AND IRON TUBING.

Our Own Experiments.

Materials.—Our materials consisted of steel skelp, (1) of 1897 and (2) of 1906, in each case selected by us at the works of the National Tube Company, and (3) of wrought iron skelp ordered by us from works of high standing. The 1906 steel had been Spellerized. How far its properties are due to this treatment and how far to other modern features of manufacture we do not know.

Conditions of Our Tests.—First twelve pieces of steel skelp of 1906 and ten of wrought iron of 1906, each 8 x 12 ins., were immersed side by side, insulated from each other in wooden crates, for 224 days in hot aerated artificial sea water, a condition which in previous tests has usually appeared the most unfavorable to steel. Then the same pieces of steel skelp were re-exposed in like manner for six months along with twelve pieces of steel skelp of 1897. Thus the time of exposure was as follows:

Wrought iron of 1906	7	months
Steel skelp of 1906	13	"
Steel skelp of 1897	6	"

All our results are given in Tables I and II.

A. Comparison Between Our 1906 *Steel and Wrought Iron Skelp.*—Though the loss of weight in seven months of these two classes of skelp was practically identical, yet the wrought iron skelp pitted in seven months much deeper than the steel did in thirteen months. It seems to us that the fairest way is to confine our attention to the deepest pit in each plate, because, as has well been said here, if there is a hole the water will run out, no matter how much the pipe weighs. Using thus the deepest pit in each piece as a basis of comparison, in our tests the steel pitted very much less than the wrought iron. Thus nine out of the twelve steel pieces pitted very much less deeply than any of the wrought iron pieces.

The remaining three pieces of steel pitted only very slightly deeper than the very best of the wrought iron pieces.

TABLE I.—SUMMARY OF TESTS ON LOSS OF WEIGHT BY CORROSION.

No.	Date.	Exposed to	Material Tested.	No. of Pieces.	Size of Pieces, in Inches, Approximately.	Length of Exposure.	Relative Loss of Weight by Corrosion.
1.	1897	Sea-water	Steel skelp	11	16x24	2 yrs.	117 per 100 of wrought iron
			W. I. skelp	12	"		100 " " " "
2.	"	River-water	Steel skelp	12	"	"	94 " " " "
			W. I. skelp	12	"		100 " " " "
3.	"	Weather	Steel skelp	12	"	"	103 " " " "
			W. I. skelp	12	"		100 " " " "
4.	"	"	Steel skelp	12	"	8 yrs.	133 " " " "
			W. I. skelp	12	"		100 " " " "
5.	1906	Hot aerated sea water (artificial)	Spellerized steel skelp of 1906	12	8x12	224 da.	101 " " " "
			W. I. skelp	10	"		100 " " " "
6.	"	"	Spellerized steel skelp of 1906	12	"	6 mos.	100 per 100 of spellerized
			Steel skelp of 1897	12	"		109 " " "

NOTE.—The details of Tests 1 to 3 inclusive are given in the "Proceedings of the International Congress on Methods of Testing Materials of Construction," held at Paris, July 9 to 16, 1900. "Effets Relatifs de la Corrosion sur le Fer, l'Acier Doux et l'Acier au Nickel," H. M. Howe, Paris, Vve. Ch. Dunod, 49, Quai des Grands-Augustins.

B. Comparison of Our 1906 *Steel Skelp with the Steel Skelp of* 1897.—The 1897 steel lost in six months about 9 per cent. more by weight than the 1906 steel lost in six months, and pitted in six months very much deeper than the 1906 steel did in thirteen months. Nine out of the twelve 1906 pieces pitted (in their total exposure of thirteen months) 0.005 in. or less. Only two of the 1897 pieces (in their six-month exposure) pitted as little as this, and half of them pitted from four to twelve times as deep. The deepest pit in any of the 1906 pieces was 0.014 in. Only six out of the twelve 1897 pieces pitted as little as this, and of the remaining six, four pitted at least half deeper than this, and one pitted more than four times as deep.

C. Previous Tests by One of Us.—In some tests begun in 1897, on a scale some twenty-five times as large as that of any previous tests, one of us found that the steel skelp of 1897 lost

17 per cent. by weight more than wrought iron in sea water, 6 per cent. less than wrought iron in river water and 3 per cent. more than wrought iron on exposure to the weather. On prolonging

TABLE II.—THE PITTING OF STEEL AND WROUGHT IRON SKELP.

Steel Skelp, 1897, National Tube Co., Immersed 6 Months.		Spellerized Steel Skelp, 1906, National Tube Co., Immersed 13 Months.		Wrought Iron, 1906, Immersed 7 Months.	
Mark.	Pitting, Depth of Deepest Pit, Ins.	Mark.	Pitting, Depth of Deepest Pit, Ins.	Mark.	Pitting, Depth of Deepest Pit, Ins.
....	0.008	X 18	0.014	D 11	0.022
11	0.022	Y 17	0.005	D 12	0.052
116.1	0.062	Z 16	0.005	DD 6	0.025
106.2	0.007	O	0.005	DD 9	0.022
106.3	0.018	OO	0.005	DD 10	0.010
107.2	0.005	OOO	0.005	DDD 7	0.031
108.1	0.021	CC 1	0.005	DDD 8	0.029
109.1	0.016	CC 2	0.005	DPP	0.026
109.2	0.010	CC 3	0.005	FPP	0.035
112.1	0.012	CCC 5	0.012		
114.1	0.020	CC O	0.005		
114.2	0.005	C 4	0.009		

the exposure to the air to a total length of eight years, the steel compared less favorably with the wrought iron, and lost 33 per cent. more than it. These results are condensed in Table I.

EXPERIMENTS OF OTHERS.

D. Principal T. N. Thomson,*, in a direct competitive test, found that three steel pipes 2 ins. in diameter and about 1 ft. long pitted somewhat less in hot water than three wrought iron ones tested together with the steel under conditions which should insure perfect fairness, in a test which lasted for about a year. His extended investigation into the actual behavior of piping in service led him to doubt whether steel was more subject to pitting than wrought iron.

These experiments have been criticized on the ground that the coupling together of steel and wrought iron tubes would lead to

* "Relative Corrosion of Wrought Iron and Soft Steel Pipes," presented at the 1908 meeting of the Society of Heating and Ventilating Engineers, by T. N. Thomson, Principal of School of Sanitary Engineering, International Correspondence Schools, Scranton, Pa.

galvanic action between these dissimilar metals, which might mask the normal corrosion. To this there is an obvious reply, viz., that in a very great variety of engineering structures like galvanic action has been at work in the past, is still at work, and is likely to continue, such as the action between the wrought iron couplings and the pipes themselves, in pipe lines; between the tubes, rivets, and sheets in boilers; between the rivets, the tension, and the compression members, and sometimes the rails, in bridges, etc.

While this reply has some force, a much more conclusive one is that, on the theory that the steel would have pitted more than the wrought iron if they had not been in galvanic contact with each other, and that this deeper pitting of the steel was prevented by that contact, then the deepest pitting of the steel should have been in the middle of each length, where the metal was farthest away from the galvanic contact, and hence where the alleged protective action of the wrought iron would be the weakest. If, on the other hand, it is alleged that the corrosion of the wrought iron was hastened by this contact with the steel, then the deepest pitting of the wrought iron pipes should be at their ends where they adjoin the steel pipes.

But this is not true. Hence we infer that there is no excessive corrosiveness of the steel masked by the galvanic connection. Thus Principal Thomson informs us that, judging by the naked eye, the lengthwise distribution of the deepest pitting is very even. Such indication of unevenness as he finds is the reverse of that here called for, and hence, if it is taken to have any meaning, it indicates that the galvanic action has hastened instead of retarding the pitting of the steel pipes. Thus he finds "perhaps, a slight exception in some of the steel samples where the ends seem to be a little more deeply pitted than the centers."

E. In the Sulphuric-Acid-Bearing Water of Coal Mines four Spellerized steel pipes pitted very much less than any of four wrought iron pipes of several makers tested alongside. The steel and iron were separated from each other, and held in wooden crates, wholly submerged. In each of three other tests, the conditions of which are not fully given, steel pitted very much less than iron. These tests were carried to the destruction of the iron pipes by pitting. They are reported in the *Iron Age* of July 12, 1906.

F. The Same.—Seven pieces of $2\frac{1}{2}$-in. pipe, three of them of

uncoated steel, three of uncoated iron, and one of coated steel, coupled in series, had strong sulphuric acid coal mine water pumped through them. They were found to have been pitted or corroded through after the following periods:

Plain steel,	from 42	to 54	days,	average,	47.3	days.
Plain iron,	" 49	" 62	"	"	53.3	"
Coated steel,				"	92	"

G. The Same.—One piece of plain steel and one of plain iron, exposed inside and out to running sulphur water, were found eaten through after forty-eight days. The bottom of the steel pipe was completely gone, that of the iron pipe "was eaten full of holes." Here the steel seems to have corroded faster than the iron, though there is no evidence as to which pitted through first. The information under *F* and *G* comes to us direct from the general superintendent of the company which owns the coal mines.

H. The Same.—The superintendent of a bituminous coal mine immersed four pieces of steel and wrought iron tubing, each about 1 ft. long and insulated with wooden sticks, in the water-discharge main from the mine, for sixteen days. "The wrought iron was pitted and worthless, while the steel tubing was worn down on the end where the water first entered to a very sharp edge, but there were no holes in same. We made another test similar to this with the same results."

The corrosiveness of the water here is shown by the fact that wrought iron pipes, set in a perforated wooden box and submerged for fourteen days in the sump, disappeared completely, and that "brass fittings would not last thirty days."

I. The Same.—A pamphlet issued by the Reading Iron Company quotes from a supply agent who testifies that, in "one or two instances," iron and steel pipes have been set side by side with sulphuric mine water passing through them, and the "steel pipe had been so badly eaten on the bottom. . . . that it was as thin as paper, whereas the iron pipe, although badly corroded, retained almost its original thickness." Passing by the evident superficialness of the knowledge of the conditions implied by the writer's ignorance as to whether the number of tests was one or two, the report should be received with caution. The fact that the steel pipe was as thin as paper suggests that its corrosion was

uniform rather than by pitting. Further, the description of the behavior of the iron pipe is hard to understand. The fact that, though retaining almost its original thickness, it was badly corroded, on its face seems to imply that the iron pipe had pitted badly; otherwise it is hard to understand how it can both have been badly corroded and yet have retained almost its original thickness. It might thus be inferred that iron pitted here but steel did not; but no strong inference can be drawn.

J. Interlocking and Signal Systems.—We learn that twenty-nine pipes, all believed to be wrought iron, after long use in the interlocking and signal systems of a very important railroad, were lately examined, with the result that twelve were found to be steel and only seventeen iron. The life of the steel pipes was in this case somewhat longer than that of the iron ones. Thus, of those which were practically destroyed by corrosion and pitting,

11 were of steel with an average life of 13.5 years.
8 " " iron " " " " " 10.4 "

Our information comes direct to us from the general superintendent of the railroad.

K. Locomotive Boiler Tubes.—A late officer of a very important railroad informs us directly that in tests between Spellerized steel and charcoal iron locomotive boiler tubes, in engines of the same kind and service, the Spellerized steel had "longer life" than the iron. We understand that these tests were carried to destruction.

KK. The Same.—The superintendent of motive power of an important railroad, on which acid water has corroded the boiler tubes and especially "the bead and joint made in the firebox flue sheet," reports to us directly that "we can say with a good deal of certainty that there is practically no difference between the iron and steel, at least as far as we have been able to determine, one faring just about as badly as the other."

L. Stationary-Boiler Tubes.—The general manager of an important boiler company which, on account of serious pitting, had occasion to examine the behavior of iron and steel carefully, informs us directly that, "We have had reports that charcoal iron did not last as long as cold-drawn seamless steel, but we have since found inaccuracies in the records and our judgment is that

there is no practical difference." These tests are carried to destruction.

M. Note on Cases E, F, G, H, J and K.—These cases carry greater weight than our own (*A* and *B*) in one respect, viz., they were pushed to destruction. To the fact that they represent actual service we attach little weight, because of the very great difference between different sets of service conditions. The conditions of service in sulphuric-acid mine water are approached much more closely by a laboratory test with like acidulated water than by a service test with alkaline water. But the fact that these tests were pushed to destruction increases their value very greatly. One* of us has shown reason to expect that the changing conditions in the course of corrosion might change or even reverse the relative resistance of two competing substances, so that the one which at first was the more resistant of the two might later become the less resistant. An illustration of this principle is shown by the results under *C*. Here the 1897 steel, which in the first two years' exposure behaved substantially like the competing wrought iron, corroded much more than the wrought iron when that exposure was prolonged to eight years. The life of the steel was determined by loss in weight.

N. Damp Ashes.—Mr. F. W. Stone, manager of three gas companies in Ashtabula, Ohio, and Greenville, Pa., buried sixteen pieces of steel and wrought iron pipe about 4 ft. long and either $1\frac{1}{4}$ or 2 ins. in diameter, with their ends tightly closed with cement, in a box filled with ashes, which were sprinkled with water about once a week, from November, 1906, till October, 1907, and again till March, 1908, a total of sixteen months. After the second exposure the pipes were boiled in caustic potash in order to remove the rust. "It was then seen that the wrought iron pipe was very badly eaten in places, in some points being almost half way through. At other points where the pipe was protected by the coating of mill scales hardly any corrosion had taken place. The steel pipe was pitted to a certain extent all over but the pittings were not of a very large size and were not very deep."

O. Gayley Blast Drying Coils at the Isabella Furnaces. In one of the blast-drying coils here there are eight 2-in. steel tubes

* Volume VI., pp. 157–8. Also Proceedings International Congress on Methods of Testing, etc. Vol. II., Part 1, p. 261, Paris, 1900.

20 ft. long, and alternated with them eight like wrought iron tubes, all of which are "constantly covered with a thin skin of water." After this exposure had lasted more than twenty-six months, no difference could be detected between the steel and iron pipes. The Chief Engineer, Mr. Bruce Walter, says "In fact I am positive that no one who did not know the conditions would discover that there were two kinds of pipe in the coil." The tubes had "scaled very little and that uniformly."

P. Re-Use After Trial.—We have known of many cases in which intelligent and careful users, under whose conditions perishableness would outweigh any moderate saving in first cost, have, after competitive trials, used modern steel pipe again.

Q. Rusting in the Air.—The pamphlet of the Reading Iron Company, already quoted, gives illustrations of the condition of a piece of steel casing and of a piece of iron casing, exposed to the air under identical conditions. The steel was practically destroyed by corrosion after eight years, the iron was in perfect condition after eighteen years, with the sole exception that the threads were rusted, though not so seriously but that with a little oil they could be used in new couplings. This may be true of a certain lot of very bad steel and a lot of very remarkable iron, but it is absolutely incredible as applied to steel and iron as a whole. Whatever difference there may be between iron and steel, it is a matter of common notoriety that it is not of this degree. We do not mean to question the honesty of the writer's intention, but it is evident to anybody who is familiar with the subject that to make any general application of his statement would be absurd. He doth protest too much.

R. Steel Conduits.—We may here consider the admirable report of Mr. R. H. Gaines* on the corrosion of the Rochester and other steel conduits. The Rochester steel conduit, laid in 1893-4, has given much trouble from corrosion, though a wrought iron conduit near it laid in 1873-5 has not.

This case is not actually pertinent to our present inquiry, (1) because the life of a conduit depends primarily on the integrity of it's coating; (2), because the steel of which this Rochester conduit was made differs radically from modern tube steel in com-

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* *Engineering News*, May 28, 1908, p. 578.

position, with more than twice as much carbon; and (3), because the steel was made about thirteen years ago, before the present precautions against pitting were understood. But we may examine this case because every failure of every kind of steel raises against every other kind of steel a certain presumption, which is likely to be inversely as the fairness of the reader's mind, though different steels differ incomparably more in corrosion than good tube steel can possibly differ from good iron.

Though in wet soil, many steel plates which had long lost their protective coating were uncorroded, and four-fifths of the steel plates were in good condition. Thus even in 1893 steel makers could make four-fifths of their steel so that it would not corrode even under these trying conditions.

Of the eight other steel conduits discussed by Mr. Gaines, five seem to have resisted corrosion well, except that one corroded near an electric power house. Four of these are between 5 and (about) 18 years old. Of the three which did not resist corrosion well, there is one the short life of which should not carry weight. This conduit ran through the salt marshes near Atlantic City, and its short life is easily explained by its very corrosive environent. A second, that of Portland, Oregon, was made in 1893-4, and therefore was not of modern steel. The rapid corrosion of the third, the 40-in. conduit of Cambridge, Massachusetts, was evidently caused by severe electrolytic action which was destroying both wrought iron and steel.* Therefore this case raises no presumption against steel.

To sum this up, there is no evidence here unfavorable to modern tube steel. Indeed, the long life of the greater part of the steel here represented, often in spite of unfavorable conditions, is further evidence that the resistance of steel is not necessarily less than that of wrought iron, for instance in the way in which the resistance of both these varieties is less than that of cast iron. Instead, this evidence supports the already well-supported view

* "Annual Report of the Water Board of the City of Cambridge," 1904, p. 13. "The action of the electric current (liberated in the ground by the operation of the Boston Elevated Railway Company), upon the main pipes, has caused rapid deterioration, resulting in many instances in complete perforation of the pipe, thereby causing leaks which appeared at the surface of the ground."........ "Some method of relief must be devised, if the *iron and steel pipes* in the ground are to be saved from destruction." (The italics are ours.)

that corrodibility is a defect of certain individual pieces of steel or iron, a thing to be corrected by regulating manufacture and inspection so that corrodible pieces shall be rejected.

S. Competence of Plumbers to Distinguish Steel from Iron Tubing.—One of us submitted several pieces of tube which he knew by his own etching tests to be steel, and others which he knew to be wrought iron, to four different plumbers and asked them to tell, by cutting with a die or otherwise, which were steel and which iron. One plumber, after making several attempts, admitted that he could not tell. The three others reached the results given in the following table. In every case the plumber made the tests with his own dies.

TABLE III.—TRUSTWORTHINESS OF THREADING TEST WITH COMMON DIES FOR DISTINGUISHING STEEL FROM IRON TUBING.

Plumber No.	No. of Tubes Tested.		No. Reported Right.		No. Reported Wrong.		Percentage of Errors.	
	Iron.	Steel.	Iron.	Steel.	Iron.	Steel.	Iron.	Steel.
1	11	11	6	10	5	1	45	9
2	6	6	0	6	6	0	100	0
3	6	6	0	6	6	0	100	0

From the last two columns of Table III we see that, while plumber No. 1 came a little nearer the truth than if he had simply decided by tossing a penny, Nos. 2 and 3 came no nearer than if they had followed that method. No. 3, whom one of us knows well to be an unusually intelligent mechanic, took three quarters of an hour to test the twelve tubes, and included in his test very careful inspection for blisters and other indications, and also testing the ductility of the chips. He and his helpers were very sure that the power required for cutting the steel was very much greater than that needed for cutting iron, and that steel chips were very much more brittle than iron chips. He was perfectly confident that all of the tubes were steel, though in fact six of them were iron.

This goes to show that this test of threading with a die (supposed to distinguish steel from wrought iron by the greater pressure needed for threading the former), the only ready test which the plumber has, is not trustworthy. This had been shown by

Principal Thomson in his paper on "Power Required to Thread, Twist, and Split Wrought Iron and Steel Pipe."* He found that, while on an average of all his cases threading needed more power in case of steel than of iron, yet in no less than five out of the nine sets of tests, the easiest threading steel pipe needed less power than the hardest threading iron pipe. For instance, with one type of dies, the pressure needed for threading was only 100 lbs. in case of two of his four 1-in. steel pipes, but was materially greater in case of all eight of the 1-in. iron pipes tested, and was between 120 and 123 lbs. in case of six of them.

Again, trustworthy reports have been made to us of cases, first, in which intelligent and experienced engineers, to whom samples of steel and iron pipe have been submitted, have decided promptly and positively, but incorrectly, as to which was iron and which steel; and second, of cases in which pitted pipes though reported to be steel have proved to be iron.

T. Steel Tubing Sold as Wrought Iron.—If the user has not been able to tell by his own observation or that of his employees whether any given pipes were steel or iron, could he rely on the representations of the dealer from whom he bought those pipes? Here, again, the investigations of Principal Thomson are of value. Though about three-fourths of all the welded pipes made in this country are steel, he found but few contractors who would admit that they had any steel pipes. The others represented that they had iron pipe only.

It may be that, since Principal Thomson made his inquiries, dealers and plumbers have come to a better knowledge of the facts. Most of those whom we asked admitted that they had steel pipe, and six even admitted that they had no wrought iron pipe. Our inquiries were confined to New York State. But although the case may be better now than formerly, it is evidently bad enough. We bought from nineteen different plumbers' shops nineteen different pieces of pipe, which we were assured were wrought iron. On examining them we found that thirteen out of the nineteen were of steel.

U. Inconclusive Evidence.—In the cases which have been reported of the short life of steel pipes there is, so far as we have

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* Twelfth Annual Meeting of the American Society of Heating and Ventilating Engineers. January, 1906.

noticed, rarely any trustworthy evidence to show that iron would have lasted longer. It is true that in some cases iron had lasted longer on previous occasions, but such evidence as this is to be received with great caution. Conditions change from time to time. The introduction of steel pipes came along with the development of stray electrolyzing currents from the extensive applications of electricity, and with the increase in the sulphurous acid in our cities caused by the growth of manufactures and the increased consumption of coal.

Thus a new line of pipe at Joliet, Illinois, was pitted through in a year by the very local water which had been carried in iron pipes in the same mill for fifteen years without trouble. It was inferred that this rapid pitting must be due to using steel pipe, but it was found on examination that the pipe was wrought iron, and that the quick pitting was due apparently to an unsuspected increase in the quantity of sewerage.*

V. *Pitting and Uniform Corrosion.*—The common belief that wrought iron always corrodes uniformly and evenly and that steel always pits, seems to have no sound foundation. Either variety if ill-made may pit. That well-made wrought iron like steel pits, and that well-made steel is not more subject than wrought iron to pitting is indicated by the results under *A*, *D*, *E*, and less directly by those under *I* and *U*. Instances of the pitting of wrought iron are given under *F*, *G*, *H*, *J*, *L*, and *N*. The late failure of the Waterloo wrought iron standpipe, twenty-two years old, was evidently due to pitting.†

* *Engineering Record*, LI., No. 23.

† H. J. Morrison, *The Engineering News*, June 25, 1908, p. 681.

GENERAL DISCUSSION ON CORROSION.

Mr. De Wyrall.

MR. CYRIL DE WYRALL.—I am unable to agree with Mr. Cushman in his theory that there is no difference between electrolysis and corrosion. My experience leads me to believe that there is a vast difference, physically, between the two. Corrosion is a slow rotting or wasting away of the metal which first appears in small spots and gradually spreads until the surface of the metal is covered with rust. This absorbs moisture and other material from the surrounding air to such an extent that a scale of rust removed from a column or girder of steel, measuring $\frac{1}{8}$ in. in thickness shows a loss of only $\frac{1}{64}$ in. of metal. Electrolysis, on the other hand, causes a rapid disintegration of the metal. The pieces removed are in a laminated condition, and the loss of metal is equal to the thickness of the scale removed. Electrolytic action is not confined to steel and iron alone. I inspected some column foundations which were being renewed and found in every instance —some two hundred in all—the same effect, viz., the foundations, whether of brick or concrete, were cracked, the concrete disintegrated, the brick either pulverized or burned black, and in the voids formed there was a greenish bluish fluid which gave off an odor similar to that of a battery room. This (exhibiting specimen) is one of the bolts which was taken from one of the column foundations. It is, as you see, $2\frac{1}{2}$ in. in diameter, but you will notice that the greater part of it is not thicker than three quarters of an inch. You will notice the peculiar color of the rust—it is different from ordinary corrosion and the scales removed are magnetic. The column bases in this case were at ground level and were protected by an outside plate, the space between the column and the outside plate being filled with an asphaltic compound, so that the electric current had to pass down through the bolts and out of the foundation piers. I was asked to inspect another structure where the column bases were buried about two feet below grade and the earth filled in and around the lattice columns. On exposing the bases and the nuts, they were found to be in good condition, but the columns at the ground line were nearly all eaten away. A layer

$\frac{9}{32}$ in. thick was removed from each side of the web of the column, which was originally only $\frac{3}{4}$ in. thick. Fortunately, owing to the short span of the bridge, there was practically no shear or I think it would not have stood up as long as it did. This occurred on a road that had been changed from steam to electricity less than two years ago. The effect of electrolysis is going to be a source of great anxiety to engineers, and some remedy must be found to stop the cause, or nullify its action. As electrolytic action only takes place where the current leaves the structure and enters the earth, or rather, since it cannot take place unless it does leave the structure, I would suggest as one remedy, that the foundation bolts be encased in a thick vitrified pipe—sewer pipe, for instance—the bottom of the pipe being fitted into a piece molded to suit the joints and filled with some asphaltic substance. The space between the bolt and the pipe might be filled with neat cement, the whole placed in a concrete pier, and the pier protected with some non-conductive waterproofing material. This will eliminate the cause of corrosion in many instances and will mean a saving in the long run. It is the only effective method yet known, since it has been proven that, no matter how well a structure is bonded, loss of current and its accompaniment, electrolysis, will occur. **Mr. De Wyrall.**

MR. GEORGE SCHUHMANN.—Without any intention to belittle scientific research, or the valuable aid that laboratory experiments have given to manufacturing processes of various kinds, I only wish to emphasize the fact that in the matter of the corrosion of iron and steel there are yet so many unknown factors that it appears to me that, as long as the nature of these unknown factors is not fully understood, it will be impossible to exactly reproduce them in the laboratory. Nor do I believe that tests made with concentrated solutions will form a reliable guide as to what the same pieces of metal will do under the very slow corrosive influences to which, in the large majority of cases, the metal is subjected in actual service. It is like drawing conclusions from hot-house horticulture as to what the same plants would do when expose to the elements at all seasons. We can form much more reliable conclusions by comparing the plants that have actually been exposed to all kinds of weather for a long time, and even then we must not allow ourselves to be misled by a few isolated failures, but we must take the majority of cases as a guide. **Mr. Schuhmann.**

Mr. Schuhmann.

I understand that modern science attributes all chemical changes to electrical influences. It is stated that even the flame of a candle is an electric battery. If science can prove that combustion is an electrical phenomenon, it is not hard to believe that corrosion (which is really slow combustion) is also due to electrical influences.

On the other hand, without necessarily knowing anything about the electrical part of it, practice has taught us how to accelerate or retard combustion by facilitating or obstructing combination with oxygen; so practice has also taught us that certain metals will resist ordinary corrosion better than some other metals; and it will not do for our scientific friends to thrust this opinion aside just because no complete scientific explanation is given with it. In this case practice is simply stating the facts, and it is the business of the scientific men to make their theories agree with the facts.

I need not go outside the Proceedings of this Society to prove that the testimony of the practical men is decidedly in favor of wrought iron over steel, as far as resistance to corrosion is concerned. We must not forget, however, that there are different kinds of wrougut iron, as has already been pointed out by several members at a meeting of the Society two years ago. The gist of their opinions is that wrought iron made from junk-yard scrap by the so-called "busheling" process does not resist corrosion nearly as well as wrought iron made from pig iron by the "puddling" process. It is unfortunate that we have not a short separate word in our present nomenclature to distinguish these two kinds of wrought iron. The chief difference between puddled wrought iron and low-carbon steel is that the former is interlaced with fine streaks of slag between the different fibers of pure iron, while steel is practically slagless. And yet, simple as this distinction is, the matter is still very much misunderstood by many consumers of iron and steel, and, to make confusion worse confounded, some manufacturers of steel pipe call their product "Wrought Pipe." As it was doubtless due to this perplexing name that thirteen out the nineteen plumbers sent steel pipe to Professors Howe and Stoughton, under the impression that they were sending wrought iron pipe, they had evidently purchased the same as wrought iron pipe. If the claim of the makers of steel pipe as to the superiority of their product over wrought iron pipe is true, why

should they object to calling their pipe "steel pipe" instead of calling it "wrought pipe," which misleads most of the average consumers? Mr. Schuhmann.

We all know how the thinnest film of oil will prevent ink from taking hold on paper; so the glass-like slag streaks between the iron fibers in wrought iron will resist acid and retard corrosion. To illustrate this roughly, take several layers of blotting paper to represent the pure iron, and at one end lay between these layers of blotting paper pieces of thin writing paper to represent the slag, leaving at the other end nothing but blotting paper held tightly together to represent steel; then allow an equal quantity of ink to drop near each end of the package, and you will find that at the one end the ink has penetrated several layers of the blotting paper, but at the other end the first layer of writing paper has practically arrested the penetrating action of the ink. May not the corrosive fluid be arrested in a similar manner by the slag in wrought iron as the penetration of the ink has been arrested by the writing paper?

As regards electrolytic action, Mr. R. H. Gaines has come to the conclusion that the units of iron enclosed in thin silicious shells (which are non-conductors) behave like water-tight compartments in a ship.

Inasmuch as the busheling process also covers the pieces of scrap with slag, the question may naturally be asked, "Why should it not resist corrosion as well as puddled pig iron?" There may be two reasons:

First. There is very much less slag in scrap, and the pieces of steel that may be mixed with it are not brought to the fusion point; therefore the slag cannot penetrate the same. In other words, the water-tight compartments will be much fewer and larger, and their walls very thin.

Second. Owing to the miscellaneous mixture of junk-yard scrap, varying from high-carbon steel to old barrel hoops and tin cans, the resultant product will be a conglomerate, and the variation in the composition may, under the same provocation, create a greater difference in electrical potential than the more uniform composition of puddled pig iron, so that electrolysis will be very much accelerated thereby. However, be that as it may, the fact remains that busheled scrap will not resist corrosion as well as puddled pig iron.

Mr. Schuhmann. Various attempts have been made in times past to make iron and steel rust-proof, but none have proven effective in the long run. About twenty-four years ago the Bower-Barff process looked very promising, but was soon abandoned as far as pipes were concerned. The usual protection with a view to prolonging the life of pipe now in use is galvanizing, tar coating, and painting; and in all of these treatments wrought iron has a decided advantage over steel, owing to its rougher surface causing the coating to adhere better and in thicker layers.

I do not want to say that science may not eventually discover a way to prevent, or at least reduce to a minimum, the corrosion of iron and steel, although I fail to see how steel can ever be impregnated with the life-preserving slag fibers that exist in wrought iron. Another drawback is that it requires such a long time to demonstrate its durability in actual service. The increase of resistance to shock, such as armor plate, for instance, can be tested by a few shots at the proving grounds, and a practical man need not care to what electrical or chemical phenomena this high resistance is due, nor what "Harveyizing" or "Kruppizing" consists of, when he sees the results before him. But making steel rust-proof cannot be proven by a few shots or by laboratory tests. The test of actual service is the only convincing one, and that will require many years of demonstration.

The "Engineering News" of May 28, 1908, contains a very exhaustive and interesting article by Mr. R. H. Gaines, Chemist of the Water Supply Commission of New York, describing the results with wrought iron and steel conduit pipes in connection with the water supply of the City of Rochester, N. Y., which fully confirms my contention that only many years' service will demonstrate the question of durability thoroughly. The wrought iron conduit varies from 24 to 36 ins. in diameter and from $\frac{3}{16}$ to $\frac{1}{4}$ in. in thickness, and was laid in 1873-5. The steel conduit, 36 to 48 ins. in diameter and from $\frac{1}{4}$ to $\frac{3}{8}$ in. thick, was laid in 1890. During the first nineteen years not one cent was expended on repairs to the wrought iron conduit. Only after 1894, and from then till 1904, a few leaks due to corrosion appeared (less than ten), and none have been reported since 1904. The steel conduit laid in 1890 began to develop rust holes in 1900 and has been developing new holes ever since, so that by the end of 1907, 164 holes had been

repaired, notwithstanding the frequent digging up of the conduit for renewal of painting, japanning, tar coating, etc., with a view to prolonging its life; and this, notwithstanding the fact that the steel plate was nearly twice as thick as the iron plate. According to recent reports from Rochester, the wrought iron conduit remains generally in good condition after thirty-three years' use. Incidentally Mr. Gaines quotes similar experiences of corrosion of steel in connection with the municipal water supplies of Portland, Ore., Cambridge, Mass., New Bedford, Mass., Pittsburg and Allegheny, Pa., the East Jersey Water Co., and Atlantic City, N. J. **Mr. Schuhmann.**

Many more similar experiences can be cited, but it would be only cumulative evidence. I wish to mention, however, a few peculiar cases of corrosion of wrought iron and steel, just to show that we must not be guided by isolated failures, especially if the surrounding conditions are not fully understood.

I learned of a case where several charcoal iron tubes corroded badly in a tug-boat boiler that was out of service many months, while tubes of the same kind of metal put into other tug boats of the same company, but which were kept in service, gave good satisfaction. We had many iron tubes corrode rather quickly in the boilers of one of our blast furnaces, but after changing the feed water supply from the river to a nearby creek, the trouble ceased. I have been informed that iron and steel trolley poles will corrode faster at the ground line than either above or below, while Dr. Cushman tells us that in many steel wire fences the top strands corrode faster than those near the ground, although the latter are more moist and often covered with weeds.

All this goes to prove how puzzling this whole problem of corrosion is, and how impossible it is to demonstrate the durability of decades by a few weeks' test in a laboratory. Of what value, for instance, is the widely quoted test made by Professor Thomson who took about five feet each of 2 in. iron and steel pipe, exposed these to hot water service for a year, and then made calculations of their supposed life by taking micrometer measurements of the depth of corrosions, as compared with the evidence of actual service of larger pipes, miles in length, weighing thousands of tons, and where corrosion continued until the holes penetrated the metal?

As stated before, I do not claim that wrought iron will outlast steel under all conditions. For instance, in an acid solution that

Mr. Schuhmann. is strong enough to attack the slag, wrought iron may corrode even faster than steel, but such cases are very rare in actual service. Then there are sometimes unknown conditions that will cause wrought iron in one place to corrode faster than steel in some other place, without any apparent difference in the surroundings. In the Indiana gas belt, two parallel pipe lines were taken up on account of the gas having given out. Each line was 25 miles long and the two lines had been laid side by side, in practically the same soil, and had transported the same kind of gas. The wrought iron pipe was 8 ins. in diameter and had been in service 18 years, while the steel pipe was 6 ins. in diameter and had been in service only 11 years. While $24\frac{3}{4}$ miles (over 99 per cent.) of the wrought iron pipe was still in good condition after being taken up, 1,200 feet (or nearly 1 per cent.) was so corroded as to make it unfit for further use. On the other hand 14 miles (56 per cent.) of the steel pipe was thrown aside for the above reason, 11 miles (44 per cent.) being still in good condition. This shows that there are some soils which will corrode wrought iron, and also some soils in which even steel will survive many years. We must, therefore, take the majority of cases as our guide.

The *Engineering News* of May 28 says editorially in reference to the Rochester conduit and the greater durability of wrought iron over steel:

> Whatever the reason may be—whatever the mechanical or chemical or electrical phenomena that cause the pitting—the fact is clear; and it is with the facts that the practical engineer is concerned.

Professor Stoughton refers to modern and so-called "Spellerized" steel, without telling us, however, what this "Spellerizing" consists of. If I am not misinformed, the elimination of manganese is one of its chief features. Since manganese has been blamed by several scientists as the principal cause of corrosion in steel, it sounds plausible to say that eliminating the manganese will prevent corrosion. The only thing that remains to be done is to prove it.

I have always understood that a certain amount of manganese is necessary in steel to reduce or de-oxidize any over-oxidized iron, as well as to neutralize sulphur, and the question naturally arises whether omitting the manganese does not introduce some new

complication that will give trouble in another direction; or, to put it in other words, manganese is supposed to be the microbe of the rust disease; therefore, by "sterilizing" the steel so as to remove this microbe, rusting is prevented. It remains to be seen, however, whether this sterilizing process has not inoculated the patient with a new disease. The argument that the slag net imbedded in wrought iron has a retarding influence on corrosion is evidently ignored, but to my mind it is of more importance in preventing corrosion than the absence of manganese. This slag net can never be reproduced in steel made by the fusion process. We may as well attempt to roll out molten lead and give it the texture of a woven fabric. **Mr. Schuhmann.**

Bessemer and open-hearth steel is generally cast into large ingots weighing many tons, and the subsequent cooling causes the different ingredients to segregate, resulting in distinct differences in quality, chemical as well as physical, varying from top to bottom and from the outside to the center of the ingot, as has often been pointed out and elaborated upon before the Society, or, as Dr. Dudley stated this evening in his remarks about steel rails, "Every heat is a law unto itself."

In the puddling process the puddled ball is first rolled into bars weighing about 200 lbs. or less. These bars are then cut up, they become more or less mixed up, and the short pieces are then piled on top of each other for further re-heating and re-rolling, making a much more uniform product with practically no variations from one end of the bar to the other. This is another feature that can never be duplicated in steel which is rolled out of ingots, slabs or billets.

However, we may theorize all we want to; actual experience under all kinds of service is the only reliable guide after all. If we consider that in the Rochester steel conduit, which was evidently not "Spellerized," the steel lasted 10 years before the first perforation occurred, that it required 20 years before the thinner wrought iron had a hole, and that the wrought iron conduit has now been in use about 34 years, how can anybody prove now that this "Spellerized" steel will last as long as wrought iron, or even as long as ordinary steel, or what its condition will be ten years hence? Time alone will tell, and the final verdict will again be given by the practical men after years of actual experience.

Mr. F. N. Speller.—Two years ago, Mr. Schuhmann made **Mr. Speller.**

Mr. Speller. certain statements, by which we were to infer that all attempts to compare the life of iron and steel pipe by measured loss were unreliable, because iron corrodes uniformly, while steel corrodes locally in pits, and that it was quite hopeless, assuming this to be the case, to remedy this tendency in steel. We were not discouraged by these predictions of failure, and results obtained in the two years which have elapsed, as shown in the present review by the authors, Professors Howe and Stoughton, of experiments, and service tests which have been conducted at many places, all go to show that this inference is fundamentally in error, and that modern pipe steel actually pits less and outlasts wrought iron when compared on the same basis. The increasing percentage of steel pipe in use nowadays is significant of a change in popular opinion.

This evening Mr. Schuhmann hangs his argument on the protecting effect of cinder in wrought iron. This would be reasonable if, as he would have us believe, the grains of iron are enveloped in cinder, or, according to his later theory, that cinder lies in thin sheets, between which are layers of iron. Aside from the fact that we have only $1\frac{1}{2}$ parts of cinder to protect 98 parts by weight of iron, anyone who has looked at a polished section of wrought iron pipe under the microscope, will have seen that the cinder is very irregularly distributed in strings and patches, and that there are many loop holes, free from cinder, through which corrosion is unobstructed. Moreover, owing to the concentration of cinder in places, electrolytic action comes into play, accelerating corrosion at these points. Hence, instead of being a benefit, cinder in puddled iron seems to be the main cause of irregular corrosion.

We recognize our share of responsibility, in that steel, in order to have the highest resistance to corrosion, must be uniform and well worked. Our experiments along this line started back in 1903. Tests made prior to this date in the laboratory indicated that pitting was usually caused by electro-negative substances adhering to the surface of the metal, such as mill-scale, which was shown to have as much as 0.2 volt difference of potential to the iron. On removing this scale corrosion became considerably more uniform. It was found that steel which had been locally worked or forged gave better results than when simply rolled out. This led to the development of a process of mechanically working steel, which has come into general use in our mills. There is no myste-

rious principle involved in this treatment, and I believe the beneficial effect of such local work at proper temperature will be admitted by all. There is not time, nor is this perhaps the place, to discuss mechanical details of the process employed. Other improvements in the metallurgy of pipe steel during recent years have contributed largely in rendering the metal more uniform and weldable and at the same time have doubtless raised the standard. **Mr. Speller.**

Regarding the protection of iron from rusting, Dr. Cushman has indicated possible methods by which this may be accomplished by preventing the solution of iron. The practical application of this remains to be worked out. In the meantime, however, we can in many cases prevent the secondary reaction, i. e., oxidation, without which corrosion cannot continue to any noticeable degree. In the case of boiler waters or heaters, this may be accomplished in part by preheating the water, thus driving off the gases, or, by passing the heated water over a large surface of steel turnings or other scrap, through which agency the oxygen is removed by well-known reactions. We have applied this method with encouraging results in an experimental way.

Mr. G. W. Thompson.—I want to make a suggestion, Mr. Chairman. If we can get some definite information as to the composition of rust as formed under different conditions it seems to me that we will have practical knowledge on which it will be safe to base future calculations. For instance, the plates which were exposed at the Havre de Grace bridge were pickled first, and then buried in lime. Four of those plates were left over and were placed in a rear shed back of my laboratory for the purpose of having them rust and then collecting the rust. This rust was analyzed and outside of the hydroscopic water, which amounted to about 8 per cent., the composition was Fe_2O_3 H_2O. Then we have the case of rusting referred to by Mr. DeWyrall, which apparently involves FeO in some form. **Mr. Thompson.**

In many cases where pickling has not taken place we get a different formation due to the presence of scale or magnetic oxide, or whatever it may be, or at least iron in the form in which it cannot be readily changed by oxidation. It might be a desirable thing to subject plates of steel to oxidation both before and after cleaning, and to notice the difference in the composition of the rust. It seems to me that in the study of the products of rusting a great deal might be learned as to the actual causes of rusting. In this particular rust

Mr. Thompson. there was a small percentage of carbonic acid, about 0.17 per cent. (0.16 per cent. in one case, and 0.18 per cent. in another).

It is very important in this connection that the specific gravity of the rust itself be obtained. By a test which I have made it would appear that the pieces increase in volume, compared with the volume of the original iron, nearly five times; in other words, one volume of iron became five volumes of rust. Now if that is the case in rusting we have entirely different conditions from those where scale is present, because the scale retains its original volume. With rust there is a pitting and arching over which is so common. I would suggest, Mr. Chairman, that this line of study, the composition of rust, would be exceedingly profitable in connection with the corrosion of iron.

Mr. Schuhmann. Mr. SCHUHMANN (by letter).—The lateness of the hour prevented an oral answer to criticisms made in the paper on the pamphlet issued by the Reading Iron Co.

The first objection is to the anonymous letter from the supply agent of a coal-mining company in reference to a comparative test of wrought iron and steel pipe in a mine drain. In reply I would say that as a matter of fact corrosion will sometimes add to the volume of metal. Thus, one of the speakers in this discussion stated that he found the corroded mass was nearly five times as great in volume as the original metal. We also have made corrosion experiments with different metals and different acids, and I remember one case where the steel pipe had wasted away somewhat, and the wrought iron pipe had actually swelled up to considerably above its original thickness, and felt rather soft to the touch. It is, therefore, quite possible that the wrought iron pipe in this mine drain first wasted away, and then later corrosion swelled it up again to apparently the original thickness; and, while it would evidently to be badly corroded, it need not necessarily follow that it must be pitted on account of it. The letter was sent to us in good faith by a reliable person who desired, however, that we should withhold his name in the publication.

The second item criticised is in reference to photographs of wrought iron and steel casings. In this case the writer does give his full name and address.

I agree with Mr. Speller that the distribution of cinder in wrought iron is irregular, and doubtless has some loop-holes, allowing corrosion an unobstructed passage from one iron fiber

to an adjoining one, but it would be a very peculiar coincidence if all these loop-holes should come in line with each other through the whole thickness of the metal. It is the fact that the interlacing cinder fibers cause the corrosive fluid to take a circuitous route to find its way from one loop-hole to the other that retards corrosion in wrought iron; while steel has no such obstruction, the whole mass is a big loop-hole, and the corrosive fluid can go unobstructed in any direction that caprice may dictate. I did not claim that the cinder lies in thin sheets between layers of iron; I distinctly stated that the blotting paper experiment was only a rough illustration. If the cinder was as perfectly distributed as the writing paper between the blotting paper, I believe the ordinary corrosion would scarcely affect wrought iron at all. **Mr. Schuhmann.**

While the protective effect of this cinder or slag net is admitted, the opinion is expressed that it causes a difference of potential, which may equal or even outweigh the protective effect of it; but it is suggested that where strength can be sacrificed for incorrodibility, it may be practicable to increase this protection by increasing the quantity of cinder. If the presence of a little cinder (owing to its attributed difference of potential) increases corrosion, why should not more cinder cause a still greater difference of potential, and, consequently, increase corrosion still more instead of promoting incorrodibility? And why should not cast iron with its five to seven per cent. of impurities have a still greater difference of potential? Yet it is a well-known fact that it resists corrosion better than even wrought iron.

Mr. Speller (by letter).—Mr. Schuhmann's argument has already been answered in my previous discussion where attention was called to the relative quantity and shape in which the cinder is found in wrought iron skelp. Any one can satisfy himself of this by examining polished cross sections of this iron which will show that it is not hard to find loopholes unobstructed by cinder. As the cinder occurs in elongated strings it would indeed be strange if these cinder-free openings did not occur quite frequently. This and the difference of potential between cinder and iron and the tendency of the metal to pit, are matters which have been demonstrated experimentally, and do not rest on mere opinion which has been found to be a very unsafe guide in this question of relative corrosion. **Mr. Speller.**

Mr. Howe. MR. H. M. HOWE (by letter).—The work of accumulating and sifting evidence delayed us so greatly that the form of our paper, as originally presented, did not satisfy us. We have therefore recast it, but, we believe, without important change in its substance.

Mr. Cushman may be yielding to the very human impulse to make a favorite cause explain everything. Electrolytic action surely hastens corrosion very greatly, and in practice it may well be true that nearly the whole of corrosion is electrolytic. But imagine the case of absolutely pure iron, without stress or slip planes, immersed in pure water containing dissolved oxygen. Will not the iron go into solution till the solution-tension is reached and will not the iron so dissolved oxidize, precipitate, and leave room for more iron to dissolve? It seems to me that in this and like ways corrosion may go on without electrolysis. If so, then electrolysis is an aggravator and hastener of corrosion, but not essential to it. My criticism is directed rather against the form than against the substance of Mr. Cushman's remarks.

Mr. Schuhmann's question, if a little cinder increases corrosion by difference of potential, why should not more cinder cause a still greater difference of potential, and consequently increase corrosion still more, seems to me nearly parallel with the question, if your patent stove will save half my coal bill, why will not two stoves save the whole? The difference in color between one copper cent and one nickel five-cent piece is exactly the same as that between one thousand cents and one thousand nickels, and exactly the same is true of the difference in potential.

He asks how it is that, if the difference of potential between cinder and iron may more than offset the mechanical protective action of the cinder of wrought iron, yet cast iron corrodes less than steel? Whether such difference of potential shall or shall not outweigh mechanical protection depends on how great the difference of potential is, and how effective the mechanical protection is. In one case it may, in another it may not. Actual experiment must decide in each case.

The difference of potential between the graphite and the cementite of cast iron on one hand and the iron itself on the other, is presumably very different from that between the cinder of wrought iron and the iron. Moreover the difference between iron

and any one mechanically protecting impurity, graphite or cinder, **Mr. Howe.** will differ greatly with the environment.

There may be conditions under which the difference of potential between cinder and iron is so slight that its effect is far outweighed by the mechanical protection of the cinder. In such cases wrought iron should have a material advantage, and might be much more resistant than even the best steel.

In point of fact, it is the continuity of the skin of cast iron which makes it resist corrosion. So far as I have seen, when the skin of cast iron is removed its resistance is not so much greater than that of wrought iron and steel. It is most misleading to speak of the cinder in wrought iron as the equivalent of the skin of cast iron, because that skin is continuous, whereas the cinder in wrought iron is very far from continuous, and exists chiefly in threads and rods. It is this discontinuity that lessens its protective action. A glance at Fig. 1 of our paper shows how slight its mechanical protection may be. The impediment which it offers to corrosion is more like the resistance offered to the wind by a picket fence than that offered by a brick wall with occasional loopholes. Such an impediment might easily be outweighed by difference of potential.

He misleads his readers unwittingly by speaking of our evidence as "laboratory tests." In point of fact our conclusions were based in very large part on service tests, many of which were pushed to destruction. He explains to me privately that under "laboratory tests" he includes "all tests which deviate from average service conditions, that is, not only those that are made in the laboratory but also all tests made under such extraordinary service as to push corrosion to an unusually early destruction of the metal,"—"and that the proof of the slow actual average service is still lacking." With this explanation and with his misapprehension in supposing that all the tests which we reported were under "extraordinary service," his words which I have just quoted are intelligible, but still likely to mislead. To say that all tests under service conditions more severe than the majority of conditions, are not service tests at all but are laboratory tests, seems to me at least to strain the meaning of these words in a misleading way. The evidence which we have given in our appendix speaks for itself.

I fancy that the impression which Mr. Schuhmann wants to

Mr. Howe. convey is that our evidence is not conclusive proof that steel will last as long as iron under other service conditions which are so much less severe that iron lasts not two but twenty years. In that I should agree with him fully. Proof of equality under one set of service conditions is no proof of equality under others; but then it certainly raises a very strong presumption against there being any very great difference under those other conditions. Has he given due weight to this presumption?

Mr. Cushman. MR. A. S. CUSHMAN (by letter).—I wish to add a few words to this discussion. It is evident from Mr. De Wyrall's discussion that he does not understand what I mean by electrolysis and its relation to the problem of corrosion. He has said that electrolytic action only takes place where the current leaves the structure and enters the earth, and that it can not take place unless it does leave the structure. Mr. De Wyrall's attitude toward these new theories is undoubtedly that taken by many other practical students of iron and steel construction. I have said in my paper on electrolysis and corrosion that it is necessary, before proceeding with a discussion, to be quite sure that we are all accepting the same definition of electrolysis. I also said that while it was not my intention to combat the idea that stray currents play a contributory part in some cases of corrosion, I wished to emphatically point out that corrosion is dependent upon electrolytic changes which take place on the surface of the metal itself, which, it will be seen, is an entirely different problem. Gold and platinum do not corrode, simply for the reason that they do not pass into solution in water or other liquids and are not subjected to the electrolytic changes. Modern chemistry has shown very conclusively that whenever a metal is immersed in any sort of a solution electrolytic phenomena take place if the metal is to the slightest degree acted upon by the liquid. This is really all we mean by electrolysis taking place. It is difficult to see how anyone can carefully read the evidence which has been presented by a number of recent investigators of this interesting subject and attempt to deny in so many words that the corrosion of metals is an electrolytic problem. In reviewing this discussion I have been struck by the fact that it appears to have degenerated into a more or less bitter attack and counter attack between interests who represent the manufacture of different types of structural iron. This seems to me to be a matter

of regret because every one knows that there are good steels and bad steels just as there are good wrought irons and bad wrought irons. Mr. Schuhmann himself, in the long chapter which he has contributed to this discussion, calls attention to the fact that wrought iron varies very much in its resistance to corrosion. In describing what is known as the "busheling" process he has pointed out that such a metal could not be expected to resist corrosion as well as "puddled" pig iron. Whereas in a polemical discussion this fact may be a source of great satisfaction to the manufacturer of puddled pig iron, it is none the less difficult to see what good it does to the consumer, as in the majority of cases the consumer does not know whether the material delivered to him is wrought iron made out of puddled pig iron or wrought iron made out of something else. It appears to me that progress, which I understand is the object of all honest manufacturers, is to be gained, not as much by the exchange of sarcastic criticisms and innuendoes as by a determined effort to get at the root of the causes of the great differences which are observed in the resistance to corrosion of various kinds of metals, whether they are manufactured by a process which grades them as wrought iron, or whether, on the other hand, they are manufactured by one of the great modern pneumatic processes for making steel. **Mr. Cushman.**

PRELIMINARY PROGRAM OF TESTS OF STEEL COLUMNS TO BE EXECUTED AT U. S. WATERTOWN ARSENAL.

Introductory Statement by Major C. B. Wheeler, Commanding Officer, U. S. Watertown Arsenal. By reference to the annual reports of the Testing Laboratory at the U. S. Watertown Arsenal, published in volumes entitled "Tests of Metals" for the years from 1881 to 1906, inclusive, it will be seen that the laboratory has, almost from the beginning, attached great importance to the subject of tests of structural shapes.

The advances made in engineering practice during recent years, and the new problems introduced in this way, have brought out the necessity for increased testing facilities at the Arsenal in order to keep abreast of the problems so presented. As a first step toward these increased facilities for testing, a considerably increased appropriation was made available by the last Congress which has enabled the personnel of the laboratory to be increased, and has permitted the inauguration of a program of tests which it is hoped will prove of great interest and service to the engineering profession of the country.

To assist in the preparation of the program referred to above the services of Mr. William R. Webster, Consulting Engineer, and Professor Edgar Marburg were engaged by the Commanding Officer of the Watertown Arsenal with approval of the Chief of Ordnance, and at the suggestion of these gentlemen a meeting was held in New York on September 24, 1907, between the Arsenal authorities and a number of consulting engineers and representatives of leading manufacturing and consuming interests.

Twenty-five persons attended this meeting, and Dr. C. B. Dudley, the presiding officer, was authorized to appoint two committees for the further consideration of the program covering (*a*) the subject of Ingots, Billets, Blooms, Slabs, Forgings, etc., and (*b*) Tests of Structural Materials.

The Committee on Program of Tests of Structural Materials is constituted as follows:—

Major C. B. Wheeler, Chairman,
Commanding Officer, U. S. Watertown Arsenal, *ex-officio.*

Mr. J. E. Howard, In Charge of Testing Materials, Watertown Arsenal, *ex-officio.*

American Bridge Company,
Mr. C. W. Bryan, Chief Engineer, American Bridge Company of New York.
Mr. Paul L. Wolfel, Consulting Engineer, American Bridge Company of New York.

Department of Bridges, New York City,
Mr. L. S. Moisseff, Assistant Engineer to Commissioner of Bridges.

Pennsylvania Steel Company,
Mr. F. C. Kunz, Chief Engineer, Bridge and Construction Department.

Mr. Lincoln Bush, Chief Engineer, Delaware, Lackawanna and Western Railroad.

Mr. H. W. Hodge, of Boller and Hodge, Consulting Engineers.

Mr. Gaetano Lanza, Professor of Mechanical Engineering, Massachusetts Institute of Technology.

Mr. Edgar Marburg, Professor of Civil Engineering, University of Pennsylvania.

Mr. George F. Swain, Professor of Civil Engineering, Massachusetts Institute of Technology.

Mr. William R. Webster, Consulting Engineer.

A preliminary program on proposed tests of steel columns has been recommended by this Committee and it is their desire to submit it to the American Society for Testing Materials for its critical discussion, so that full advantage may be taken of any valuable suggestions emanating therefrom.

It is the desire of the testing laboratory to make such tests on structural materials as will best meet the needs of the engineering profession, and it was with this end in view that the assistance of the gentlemen referred to above as members of the Committee on Program of Tests of Structural Materials was asked.

It is requested that the discussion of the matter before the American Society for Testing Materials shall include not only the

subject of the preliminary program already adopted by the Committee, but also suggestions as to the lines along which this program should be extended in the future.

PRELIMINARY PROGRAM OF TESTS FOR STEEL COLUMNS.

NOTE.—The conference held in New York on September 24, above referred to by Major Wheeler, was devoted in part to the discussion of a proposed general program of tests of structural materials including (*a*) columns, (*b*) built tension members, (*c*) riveted splices, (*d*) connections for I beams and channels in building construction, and (*e*) the general subject of riveting. It was the sense of the meeting that tests of steel columns are of chief importance. The provisional program covering column tests was accordingly revised in the light of that discussion and otherwise elaborated. This program was then submitted to a critical discussion on the part of the Comimttee on Tests for Structural Materials, at a meeting held in New York on October 19, 1907, amended and adopted in the following form:

Introduction.

The need of an extended series of tests of steel columns has been long recognized in engineering circles. The tests made heretofore are very limited in number and have been performed generally on small sections and in part on steel of a grade no longer in current use. Test records of compression members of larger section are not only of a scattering nature, but are usually unaccompanied by adequate information as to the physical properties of the material employed. It is very gratifying, therefore, that the increased appropriation granted to the Watertown Arsenal has at last rendered possible the inauguration of a series of column tests under conditions promising a continuity of program executed under disinterested and authoritative auspices, calculated to throw much-needed light on many uncertain phases of this important subject. Unfortunately the capacity of the testing machine—800,000 pounds—imposes definite limitations from the outset upon the scope of the proposed investigation. At the conference held in New York on September 24, 1907, for the discussion of the preliminary program it was the expressed sense of the meeting that the extension of the testing facilities in this country had not kept pace with the advance in engineering construction, and a resolution was adopted by unanimous vote that the enlargement of the present

facilities at the Watertown Arsenal by the erection of a testing machine of at least 10,000,000 pounds capacity is desirable.

Proposed General Plan of Procedure.

1. Begin with a series of "standard tests," on members of rolled and built sections, designed and manufactured according to the best methods of current practice, to be tested for central loading.

2. Develop the program for a series of "special tests" under eccentric loading, and variations in design and manufacture only to a sufficient extent to fix the number of members to be tested, their length and section.

3. Place the members required for the "standard tests" under contract immediately, and place mill orders for the main sections of the members designed for "special tests," so that these sections may be rolled from the same material as those for the "standard tests."

4. Have the material for the struts consisting of single shapes shipped to Watertown as soon as possible, so that a beginning may be made with these tests.

5. While the "standard tests" are in progress develop a carefully elaborated program for the "special tests" to be submitted to the Committee at a later meeting for discussion before placing these members under contract. The experience obtained in the meantime with the "standard tests" will doubtless prove suggestive as to desirable directions of further inquiry.

Material and Workmanship.

6. Use a single grade of steel in conformity with the Specifications of the American Railway Engineering and Maintenance of Way Association for Structural Material for Bridges.

7. Adopt a limited number of rolled sections to be used singly, and as parts of built sections throughout the tests.

8. Have each section rolled from the same heat of steel, ordering a sufficient quantity to meet the requirements for both "standard" and "special" tests.

9. Have all built members of the same type manufactured at the same shop, under conditions of workmanship and inspection

required by the Specifications of the American Railway Engineering and Maintenance of Way Association. Surfaces in contact to be painted as provided for in these specifications; the members to be otherwise left free from oil and paint.

10. All rivet holes for the "standard tests" to be punched.

11. Maintain a complete record covering all details of mill and shop inspection.

12. Have test pieces 10 feet long, in triplicate, of all sizes used, shipped to Watertown for determining the elastic limit in tension and compression by extensometer, the moduli of elasticity in tension and compression, and the ultimate compressive strength.

Details of Tests.

13. Limit the variable elements to those of greatest importance, and let the number of tests be sufficient to admit of definite conclusions.

14. *Ratios of Slenderness.* Tests to embrace the following values of l/r: 25, 50, 75, 100, 125, 150, and 175. The smaller sections to be tested for the entire range and for values of l/r below 25 if desired, and the larger sections up to the highest value possible within the length limit of the machine.

15. *End Conditions.* The "standard tests" to be made for three conditions of ends, for the higher values of l/r, (*a*) pin ends, (*b*) flat ends, (*c*) fixed ends.

Tests of annular sections to be made also for the entire range of l/r with (*d*) rounded bearings on flat plates. Such bearings to be used also for a limited number of other sections.

16. *Capacity of Testing Machine.* The capacity of the testing machine in compression is 800,000 pounds. The larger sections should therefore be proportioned for an assumed ultimate strength of about 750,000 pounds, ensuring a reserve margin of 50,000 pounds.

The extreme distance between faces of end bearings is 26 feet. The lengths of the columns to be fixed in accordance with the space needed for the bearing blocks.

17. *Number of Test Pieces.* For the higher values of l/r standard tests to be made in triplicate for each of the end conditions (*a*), (*b*) and (*c*). Six additional members to be made for each value

of l/r to be used partly for tests with end conditions (*d*), but principally for "special tests" covering the following variables:

(*a*) Eccentricity of loading due to (1) shifting of pins, (2) bearings beveled in direction of either principal axis of section, and inclined in like or opposite direction at both ends, and (3) unsymmetrical distribution of rivets with respect to longitudinal axis of members.
(*b*) Columns lightly spliced at center with imperfect end contact.
(*c*) Efficiency of end batten plates and latticing; especially for eccentric loading, introducing variations in size, spacing, etc.
(*d*) Comparative efficiency of latticing and intermediate batten plates.
(*e*) Effect of tight and loose riveting.
(*f*) Effect of weight of piece in the absence of counterbalancing.
(*g*) Effect of sub-punching and reaming and of drilling rivet holes from the solid.
(*h*) Effect of ends partially fixed by riveted connections, and of round ends.

For the lowest value of l/r, tests (in triplicate) to be made only for end conditions (*a*) and (*b*) for certain sections.

18. *Measurements and Adjustments.* In connection with test with pin ends and round ends provision to be made for slow-motion adjustment horizontally and vertically and convenient means of measuring the same to the nearest 0.01 inch.

Axial deformation to be found by compressometers applied on opposite sides of the members. Deflections to be determined at centers of columns, vertically and horizontally, by delicate electrical contact devices and micrometers. Measurements for "standard tests" to be limited to the foregoing.

In connection with certain "special tests" small extensometers to be applied at various points to the main sections, batten and splice plates and lattice bars.

The above recommendations governing measurements are to be regarded only as suggestive, the details to be left to the discretion of the Testing Engineer of Watertown Arsenal.

19. *Types of Sections.* It is believed preferable to make numerous tests on columns of like section, introducing a proper number of variables, than to make a few tests on each of a wide range of types and sizes of sections.

The tests to include at the beginning:

(*a*) Annular section (welded tubes).
(*b*) New wide-flange H sections.
(*c*) I section of four angles and central web-plate.
(*d*) Double-channel section, latticed in two planes.

20. *Design of Sections.* Assuming a maximum load of 45,000 pounds per square inch, the largest permissible section will have an area of $\frac{750{,}000}{45{,}000} = 16.7$ square inches. A unit stress of 45,000 pounds will probably suffice to produce failure for all values of l/r above 25, and probably for the latter with pin ends. In case failure with flat ends is not produced under the extreme load of 800,000 pounds, pin ends may be substituted. Besides, a unit stress of 45,000 pounds is well above the elastic limit of the material, and the behavior of a member under loads beyond the elastic limit is of secondary interest. In order to use as large sections as possible, within the capacity of the machine, it is considered undesirable to assume a higher limiting unit stress than 45,000 pounds.

(Note.—The largest sectional area included in this preliminary program is only 13.71 square inches for the built I sections, owing to the length limit of the machine and the desired range of slenderness ratios.)

Design of Members. The design of the various members shown in Figs. 1 to 8, Materials Schedule No. 1, shows the number of pieces required, lengths and estimated weights. The total number of pieces is 313, and the estimated total weight 143,750 pounds.

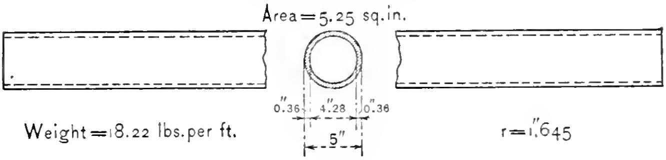

FIG. 1.—Lay's Welded Tubing.
126 Pieces.

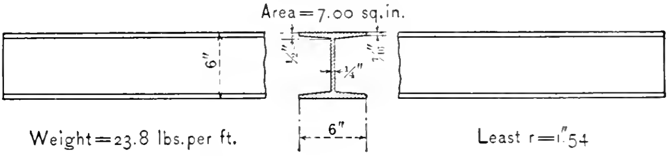

FIG. 2.—Rolled H Section.
36 Pieces.

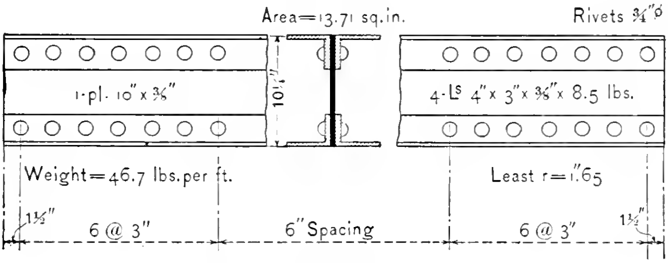

FIG. 3.—Built I Section.
81 Pieces.

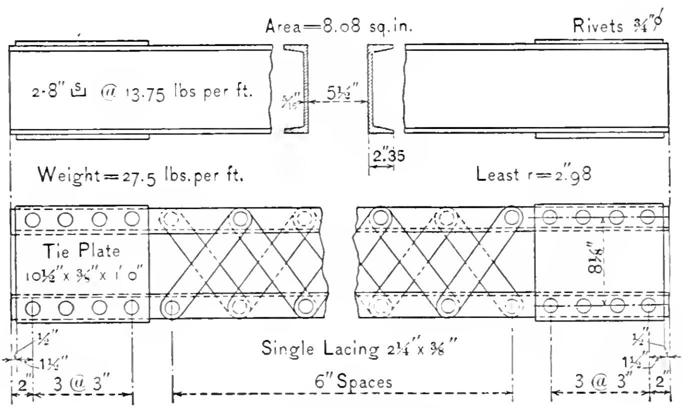

FIG. 4.—Double Channel Section.
54 Pieces.

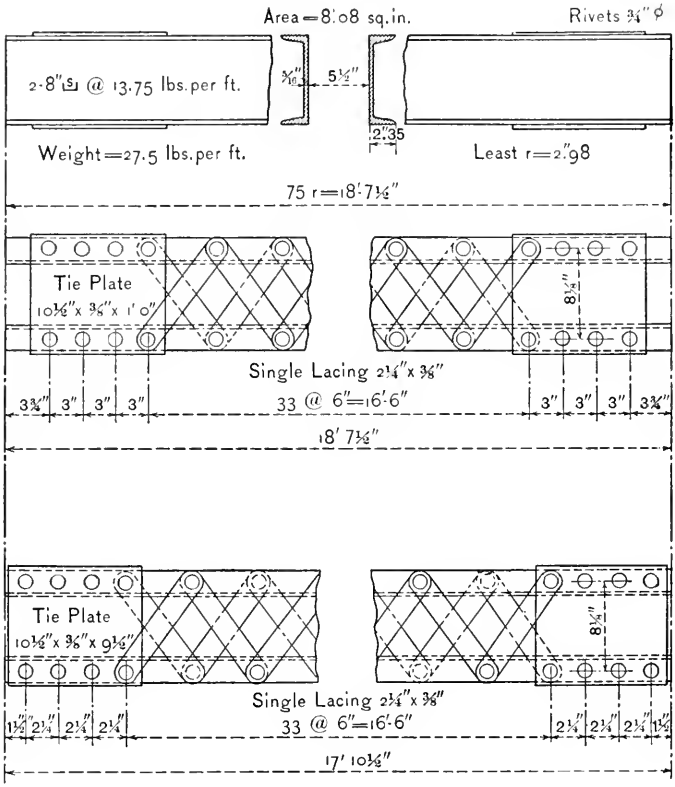

FIG. 5.—Double Channel Section.
(Single lacing, joined to tie-plate rivets.)
2 Pieces, 18'– 7½" long, for flat-end tests.
2 Pieces, 17'–10½" long, for pin-end tests.

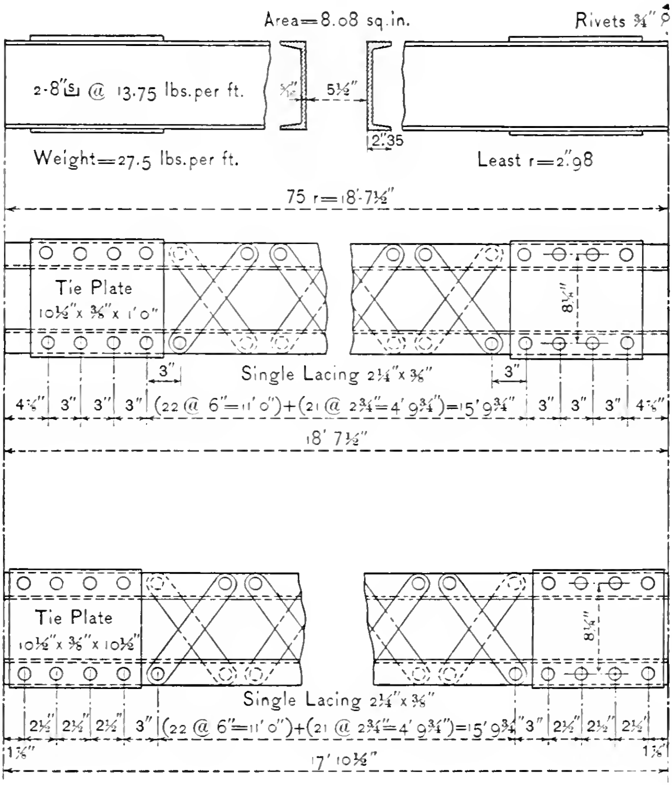

FIG. 6.—Double Channel Section.
(Single lacing, without overlapping ends.)
2 Pieces, 18′– 7½″ long, for flat-end tests.
2 Pieces, 17′–10½″ long, for pin-end tests.

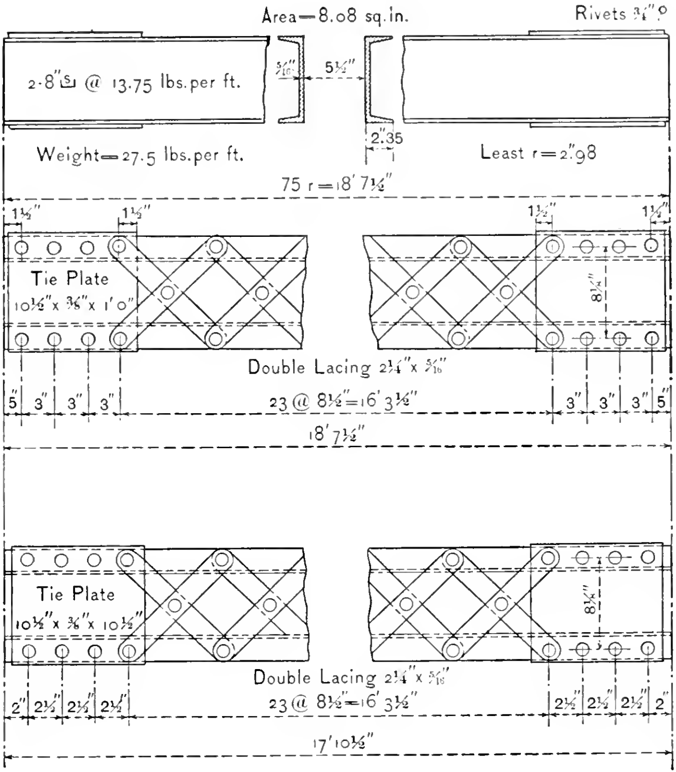

FIG. 7.—Double Channel Section.
(Double lacing, joined to tie-plate rivets.)
2 Pieces, 18′– 7½″ long, for flat-end tests.
2 Pieces, 17′–10½″ long, for pin-end tests.

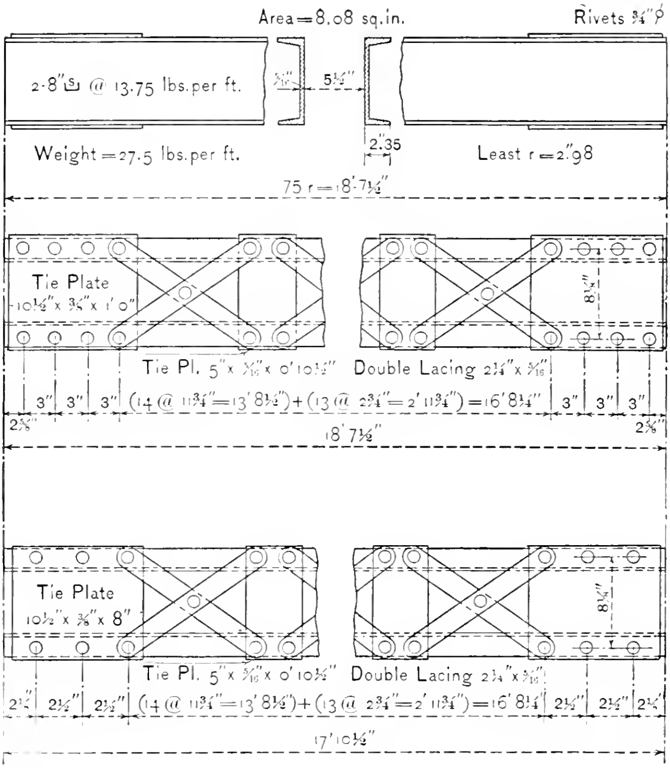

FIG. 8.—Double Channel Section.
(Double lacing, with intermediate tie-plates.)
2 Pieces, 18'– 7½" long, for flat-end tests.
2 Pieces, 17'–10½" long, for pin-end tests.

COLUMN TESTS — WATERTOWN ARSENAL

Material Schedule No. I.

Sections			End Conditions	Lengths for values of $\frac{l}{r}$ =							No. of Tests		Total Weights	Kind of Steel
Type	Dimensions and Areas	r		25	50	75	100	125	150	175	For each length	Total		
	5" diam, 0.36" tk = 5.25 □" Weight per ft. = 18.22#	1.65	Fixed	5'-5"	8'-10¼"	12'-3½"	15'-8½"	19'-1½"	22'-6¾"	26'-0"	3			
			Flat	3'-5"	6'-10¼"	10'-3½"	13'-8½"	17'-1½"	20'-6¾"	24'-0"	3			
			Pin	2'-10"	6'-3¼"	9'-8½"	13'-1½"	16'-6½"	19'-11¾"	23'-5"	3			
			Round	"	"	"	"	"	"	"	3			
			Special	"	"	"	"	"	"	"	6	126	31 300	60 000
	6"·6"-23.8#·7.0 □"	1.54	Flat	3'-2½"	6'-5"	9'-7½"	12'-10"	16'-0½"	19'-3"	—	3			
			Pin ǁ to Web	2'-5½"	5'-8"	8'-10½"	12'-1"	15'-3½"	18'-6"	—	3	36	9 300	60 000
	1-Pl. 10"·⅜" = 3.75 □" } 4-Ls 4"·3"·⅜" = 9.96 □" } 13.71 □" Weight p. ft. = 46.7#	1.65	Fixed	—	8'-10½"	12'-3¾"	15'-9"	19'-2¼"	22'-7½"	26'-0¾"	3			
			Flat	3'-5¼"	6'-10½"	10'-3¾"	13'-9"	17'-2¼"	20'-7½"	24'-0¾"	3			
			Pin ǁ to Web	2'-8¼"	6'-1½"	9'-6¾"	13'-0"	16'-5¼"	19'-10½"	23'-3¾"	3			
			Special	"	"	"	"	"	"	"	3	81	54 750	60 000
	2-8" [s-13.75# = 8.08 □"	2.98	Fixed	—	14'-5"	20'-7½"	l/r = 96 25'-10"	—	—	—	3			
			Special	—	"	"	"	—	—	—	3			
			Flat	6'-2½"	12'-5"	18'-7½"	23'-10"	—	—	—	3			
			Pin ǁ to Web	5'-5½"	11'-8"	17'-10½"	23'-1"	—	—	—	3			
			Special	"	"	"	"	—	—	—	3	54	36 200	60000
	2-8" [s-13.75# = 8.08 □"	2.98	Flat	—	—	18'-7½"	—	—	—	—	8			
			Pin ǁ to Web	—	—	17'-10½"	—	—	—	—	8	16	12 200	60 000

EXPLANATORY NOTES.

The provisions in the above preliminary program of tests of steel columns are, in the main, self-explanatory. It is thought desirable, however, to enlarge somewhat on the following features.

MATERIAL AND WORKMANSHIP. The material, with the exception of the tubes, is to conform with the Specifications for Structural Steel of the American Railway Engineering and Maintenance of Way Association which permit a range in ultimate tensile strength of 55,000 to 65,000 pounds per square inch, and prescribe a minimum percentage of elongation in 8 inches of 1,500,000 divided by the ultimate tensile strength, and chemical limits of 0.05 per cent. for Sulphur, and 0.04 and 0.08 per cent. for Phosphorus for basic and acid steel, respectively.

The requirement that only surfaces in contact shall be painted and that the members are to be left otherwise free from oil and paint is prescribed in order to aid in the observation of the yield point through the visible flaking off of mill scale.

For the tests on annular sections it is proposed to use Bessemer steel having an ultimate tensile strength well within the limits specified for open hearth steel, a minimum elongation in 8 inches of 23 per cent. and a minimum reduction of area of 55 per cent. Open-hearth steel sufficiently low in carbon to meet the welding requirements would have an ultimate tensile strength of not more than 55,000 pounds. To obviate the effect of segregation in the ingot it is proposed to adopt a somewhat more liberal percentage of discard than is customary in ordinary practice.

FORMS OF CROSS-SECTION. (*a*) *Annular Section.* This section was chosen as representing the nearest approach to an ideal distribution of metal. The results from this series of tests are expected to serve as reference standards for the values obtained for other sections of corresponding slenderness ratios.

(*b*) *Wide-Flange H Sections.* A series of tests on the new wide-flange rolled H sections is believed to be of special interest to engineers and architects. The 6-inch sections of this type are adopted as the largest available at the present time.* The program

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* Since this was written, the new Bethlehem wide-flange sections have come on the market. Some of these sections are included in Material Schedule No. 2

will be extended as may seem desirable when a larger range of these sections are brought upon the market.

(*c*) *Built I sections.* This section was selected by reason of its very common use in light building construction.

(*d*) *Double-Channel Sections.* This section was chosen as a thoroughly "typical" one, and because it offers unlimited possibilities for investigating the effect of variations in details of design. It is proposed to test the 16 columns of this type shown in Figs. 5 to 8 for $l/r = 75$, including four different styles of latticing, before proceeding with the manufacture of the columns for the more extended series of tests represented in Fig. 4. The main sections are to be rolled from the same heat of steel, but the details shown in Fig. 4 will be subject to modifications in the light of the results obtained in the preliminary tests referred to.

Slenderness Ratios. For railway bridges the slenderness ratio is commonly restricted in the best practice to 100 for main trusses and 120 for laterals. In building construction ratios of 150 are common and even that limit is not infrequently exceeded. In order to derive "constants" for compression formulas covering the entire range of the usual field in practice it was thought best to fix the upper limit of the slenderness ratio at 175 for the smaller sections. Unfortunately, the length limit of the testing machine renders the attainment of this ratio for the larger sections impracticable.

SUPPLEMENTARY PROGRAM OF TESTS OF STEEL COLUMNS.

At a meeting of the Committee on Program of Tests of Structural Materials, held at Watertown Arsenal, on April 25, 1908, the results of the flat-end tests on the annular and rolled H sections were presented, and the following extension of the program was agreed upon:

1. Verify results obtained from tests of annular and H sections for $l/r = 50$ for flat ends by making additional tests in triplicate on material available for $l/r = 12.5$, 37.5, 50 and 62.5.

2. Plot results from specimen tests in tension and compression on same sheets as results from column tests. In plotting the results of the column tests indicate the elastic limit as well as the ultimate strength for each value of l/r.

3. Order material for a complete series of tests in triplicate for built I and double-channel sections, for flat and pin ends, of (*a*) carbon steel having an ultimate tensile strength of 70,000 pounds per square inch (range 65,000 to 75,000 pounds per square inch), and (*b*) nickel steel having an ultimate tensile strength of 90,000 pounds per square inch. The nickel steel shall conform to the specifications for the Manhattan bridge. Order some additional 25-foot lengths to be utilized as may seem desirable in the light of results obtained.

Before fabricating these members, make special tests to determine the effect of workmanship, viz: (*a*) punching rivet holes, (*b*) sub-punching and reaming $\frac{1}{8}$ and $\frac{3}{16}$ inch, (*c*) drilling from the solid, (*d*) sheared edges, and (*e*) sheared and planed edges.

4. Order material for a complete series of tests up to a slenderness ratio of 350 on a built I section, fixed and pin ended, of 60,000 and 70,000-pounds carbon steel and 90,000 nickel steel. Order some additional 25-foot lengths to be utilized as may seem desirable in the light of results obtained.

5. Order material (60,000-pounds steel) for a complete series of tests on a double 8″-channel section with 12″ x $\frac{3}{8}$″ cover plate, for flat and pin ends.

Order material (60,000–pounds steel) for a few tests on a section of this type with 12″ x $\frac{1}{4}$″, 14″ x $\frac{1}{4}$″ and 16″ x $\frac{1}{4}$″ cover plates, for flat ends and a moderate ratio of slenderness.

6. Order material (60,000–pounds steel) for flat and pin-end tests on the new H shapes manufactured by the Bethlehem Steel Company, including the largest sections practicable.

7. Order material (60,000–pounds steel) for a few of the largest sections within the capacity of the machine, of rolled and built double-channel sections, with and without cover plates, to be tested with flat and pin bearings, for moderate ratios of slenderness.

8. Order material (60,000–pounds steel) for a few tests on built star sections.

9. Extend tests ultimately to cover complete series of tests on single and double-angle sections, with and without lugs ,of 60,000–pounds steel. Supplement this series of tests by special tests to establish the permissible ratio of length of unsupported outstanding leg to thickness of metal.

10. Order three extra lengths of 10 feet each of each section embraced in this schedule for the determination of the physical constants of the material.

Material Schedule No. 2 contains a summary of the number of pieces, lengths and estimated weights of the material embraced in this supplementary program. The design of these members in detail has not yet been made.

The grand summary of material is as follows: Schedule No. 1, 313 pieces, estimated weight, 143,750 pounds. Schedule No. 2, 393 pieces, estimated weight, 260,300 pounds. Total, 706 pieces, estimated weight, 404,050 pounds.

At the meeting of the Committee above referred to, a sub-committee was appointed to make a special study of the subject of latticing and to report its conclusions to the general committee.

Plate VIII.
Am. Soc. Test. Mats.
Volume VIII.
/n Arsenal—Program of
Column Tests.

Sectio		o of ests		Total	Kind of
Type	Dimen and Ar	r h th	Total	Weights	Steel
	1-Pl 10"·3/8"·3 4-∠s 4"·3"·3/8"·4 Weight p.f3		45 45	30 300 30 300	70 000 Nickel
	2-8" [s-13.75		27 27	16 700 16 700	70 000 Nickel
	1-Pl. 6"·3/8" 4-∠s $2\frac{1}{2}$"·2"·3/8"		57 57 57	22 100 22 100 22 100	60 000 70 000 Nickel
	2-8" [s-13.75 1-Pl 12"·3/8" Weight p.f 2-8" [s, 1-Pl. 12 Weight p.ft 2-8" [s, 1-Pl 12 Weight p.f 2-8" [s, 1-Pl 16 Weight p.f		24 9	19 300 3000	60 000 60 000
	1-8" H-34.6				

PLATE VIII.
PROC. AM. SOC. TEST. MATS.
VOLUME VIII.
WATERTOWN ARSENAL—PROGRAM OF
COLUMN TESTS.

COLUMN TESTS — WATERTOWN ARSENAL

Material Schedule No 2

Sections			End Conditions	Lengths for values of $\frac{l}{r}$ =													No of Tests		Total	Kind of Steel
Type	Dimensions and Areas	r		25	50	75	100	125	150	175	200	240	275	310	350	Extra	For each length	Total	Weights	
	1-Pl 10"·⅜"·3.75" } 12.71"	1.65	Flat	3'-5¼"	6'-10½"	10'-3¾"	13'-9"	17'-2¼"	20'-7½"	24'-0¾"	—	—	—	—	—	—	3			
	4-Ls 4"·3"·⅜"·9.96"		Pin ‖ to Web	2'-8¼"	6'-1½"	9'-6¾"	13'-0"	16'-5¼"	19'-10½"	23'-3¾"	—	—	—	—	—	—	3			
	Weight p ft · 46.7#		Extra	—	—	—	—	—	—	—	—	—	—	—	—	25'-0"	3	45	30 300	70 000
				Same Schedule as above														45	30 300	Nickel
	2-8" [s-13.75#·8.08"	2.98	Flat	6'-2½"	12'-5"	18'-7½"	l/r = 96 23'-10"	—	—	—	—	—	—	—	—	—	3			
			Pin ‖ to Web	5'-5½"	11'-8"	17'-10½"	23'-1"	—	—	—	—	—	—	—	—	—	3			
			Extra	—	—	—	—	—	—	—	—	—	—	—	—	25'-0"	3	27	16 700	70 000
				Same Schedule as above														27	16 700	Nickel
	1-Pl 6"·⅜" } 8.45"	0.82	Fixed	1'-8½"	3'-5"	—	6'-10"	—	10'-3"	—	13'-8"	16'-4¾"	18'-9½"	21'-2¼"	23'-11"	—	3			
	4-Ls 2½"·2"·⅜"		Pin ‖ to Web	0'-11½"	2'-8"	—	6'-1"	—	9'-6"	—	12'-11"	15'-7¾"	18'-0½"	20'-5¼"	23'-2"	—	3			
			Extra	—	—	—	—	—	—	—	—	—	—	—	—	25'-0"	3	57	22 100	60 000
				Same Schedule as above.														57	22 100	70 000
				Same Schedule as above														57	22 100	Nickel
	2-8" [s-13.75·8.1" } 12.6"	3.12	Flat	6'-6"	13'-0"	19'-6"	l/r = 92 23'-11"	—	—	—	—	—	—	—	—	—	3			
	1-Pl 12"·⅜"·4.5"		Pin	5'-9"	12'-3"	18'-9"	23'-2"	—	—	—	—	—	—	—	—	—	3	24	19 300	60 000
	Weight p ft = 42.8#																			
	2-8" [s, 1-Pl. 12"·¼"·11.1"	3.14	Flat	6'-6½"	—	—	—	—	—	—	—	—	—	—	—	—	3			
	Weight p ft · 37.7#																			
	2-8" [s, 1-Pl 14"·¼"·11.6"	3.13	Flat	6'-6¼"	—	—	—	—	—	—	—	—	—	—	—	—	3			
	Weight p. ft · 39.4#																			
	2-8" [s, 1-Pl 16"·¼"·12.1"	3.12	Flat	6'-6"	—	—	—	—	—	—	—	—	—	—	—	—	3	9	3000	60 000
	Weight p ft · 41.1																			
	1-8" H-34#·10.17"	2.01	Flat	4'-2¼"	8'-4½"	12'-6¾"	—	20'-11¼"	—	—	—	—	—	—	—	—	3			
			Pin	3'-5¼"	7'-7½"	11'-9¾"	—	20'-2¼"	—	—	—	—	—	—	—	—	3			
			Extra	—	—	—	—	—	—	—	—	—	—	—	—	25'-0"	3	27	11 900	60 000
	1-12" H-78#·22.94"	3.01	Flat	—	12'-6½"	18'-9¾"	—	—	—	—	—	—	—	—	—	—	3			
			Pin	—	11'-9½"	18'-0¾"	—	—	—	—	—	—	—	—	—	—	3			
			Extra	—	—	—	—	—	—	—	—	—	—	—	—	25'-0"	3	15	20 200	60 000
	2-15" [s-33#·19.80"	5.62	Flat	—	23'-5"	—	—	—	—	—	—	—	—	—	—	—	3			
			Pin ‖ to Web	—	22'-8"	—	—	—	—	—	—	—	—	—	—	—	3			
			Extra	—	—	—	—	—	—	—	—	—	—	—	—	25'-0"	3	9	17 800	60 000
	2-12" [s-20.5#·12.06" } 17.31"	4.79	Flat	—	—	19'-11½"	—	—	—	—	—	—	—	—	—	—	3			
	1-Pl 14"·⅜"·5.25"		Pin	—	—	19'-2½"	—	—	—	—	—	—	—	—	—	—	3	6	8000	60 000
	2-Pls 12"·⅜" } 18.96"	4.52	Flat	—	18'-10"	—	—	—	—	—	—	—	—	—	—	—	3	3	4500	60 000
	4-Ls 4"·3"·⅜"																			
	1-14"·⅜", 2-12"·5/16" Pls } 22.7"	4.80	Flat	—	20'-0"	—	—	—	—	—	—	—	—	—	—	—	3	3	5200	60 000
	4-Ls 4"·3"·⅜"																			
	4-Ls 6"·6"·½" = 23.0"	2.51	Flat	—	—	—	20'-11	—	—	—	—	—	—	—	—	—	3	3	5000	60 000
	4-Ls 4"·4"·⅜"·11.4"	1.68	Flat	—	7'-0"	—	14'-0"	—	21'-0"	—	—	—	—	—	—	—	3	9	5100	60 000

DISCUSSION.

MR. W. K. HATT.—The Watertown machine is generally supposed to be a standard testing machine for the other laboratories of the country. In view of this it is important to know whether or not this machine has been calibrated to determine its degree of error at various loads. **Mr. Hatt.**

In 1901, the speaker submitted a certain standard calibration bar to tests at the University of Illinois, Purdue University and the Watertown Arsenal. The modulus of elasticity was measured with standard extensometers and the various machines of the three laboratories. The general results were as follows:

VALUE OF E IN 100,000 UNITS.

Bar.	Watertown.	Purdue.	Illinois.	Average.
1	28.71	29.29	29.40	29.23
2	28.59	29.32	29.14	29.14
3	28.66	29.36	29.20	29.20
Average	28.66	29.22	29.33	

COMPARISON OF EXTENSOMETERS.

Extensometer.	Illinois, 200,000 Olsen.		Purdue, 300,000 Riehle.	
	Roller.	Screw—1.	Screw—2.	Roller.
Position—				
On top	29.40	29.40	29.50	.....
On bottom	29.00	29.00	.....	29.10

MR. J. E. HOWARD.—The opportunity of judging of the rating of the 800,000 lbs. Emery testing machine at Watertown is favorable in different ways. The accumulator weights which were provided with the machine when it was first installed remain the same now as formerly, and it is noted that with a given accumulator pressure a certain scale reading results, packing friction being normal. This furnishes an approximate idea of the rating of the weighing levers. **Mr. Howard.**

Several years ago a special system of levers of 50,000 lbs. capacity was procured for purposes of comparison. The rating

Mr. Howard. of the testing machine is in accord with these special levers. Furthermore, the use of modulus of elasticity bars has yielded normal results.

Mr. Churchill. MR. C. S. CHURCHILL (by letter).—The writer has carefully noted the program of tests prepared by the Committee, and has to state that in his opinion the tests proposed are satisfactory as far as the small capacity of the present testing machine at Watertown Arsenal will permit. They are, however, inadequate when we consider the heavy bridge and building columns being used at the present time.

In the opinion of the writer the tests will add but little useful information to present knowledge, and therefore a complete report of tests should not be made until a machine having a capacity of at least 5,000 tons is available. In fact, the writer is of the opinion that tests of this character had best be postponed until a testing machine of larger capacity becomes available.

Mr. Deans. MR. J. S. DEANS (by letter).—The report of the Committee in charge of this program of tests indicates that this very important question has received very careful consideration at their hands; and a comprehensive series of tests is recommended.

The demand for a larger testing machine is of such vital importance to the whole engineering profession that this fact should be emphasized in some more direct manner than by simply passing a resolution. Until this larger machine is secured, reduced models of large columns should be made and tested.

The adoption of a single grade of material and uniformity in workmanship for all tests is of prime importance. It is of equal importance to fix upon every deta'l of the actual testing in advance and adhere to the same throughout. I refer to such matters as the points for attaching extensometers, the rate and speed of the applied loads, and the principal characteristics to be observed. Readings should be taken on at least two sides of the member, opposite each other, although readings on four sides would be better. If possible one reading operator should have charge of an entire series. Unless this uniformity throughout is established, the tests made should be eliminated from any general discussion to establish formulæ, etc.

Rounded bearings are more likely to give uniform and reliable results. Testing machines are not perfect, nor are the columns to

be tested ideal; the bearing should therefore be such as to make these defects the least harmful. With rounded bearings the load is more likely to be applied axially. Mr. Deans.

The yield point should be determined carefully in all the tests. This particular characteristic of material and its value in built members can be determined more readily and with more exactness in general practice than the elastic limit, especially in the rough and hasty commercial testing. The relation between the elastic limit and yield point can be determined in the laboratory by careful experiment and this relation used in practice; i. e., after the yield point is known, the elastic limit may be ascertained by deduction.

Mr. Charles Derleth, Jr. (by letter).—The capacity of 800,000 lbs. and maximum column length of 26 ft. at present available at the Watertown Arsenal unfortunately are very low values even for the small columns proposed by the Committee. The tests will limit the values of l/r to 100 or less for the larger sectional areas. This is to be deplored, for the larger sizes included in the program are, after all, very light columns. We need information for columns of large section, on the one hand, and columns of considerable length, on the other. The program does not afford what is most desired. The committee should urge the Government to build at once a large testing machine of 10,000,000 lbs. capacity. There should be no difficulty in arranging a hydraulic machine along the lines first proposed by the late Col. Henry Flad for the testing of columns for the Eads bridge, a method referred to in Woodward's history of the St. Louis Bridge. Mr. Derleth.

The tests as outlined by the Committee though limited in scope will be valuable indeed, but it will be regrettable if such an elaborate system of tests is made without at least including columns of such sizes as usually occur (1) in ordinary steel frame buildings, say 12-in. channel columns with lacing or cover plates and lengths of 12 to 18 ft.; and (2) in average railroad bridges, say 15-in. rolled or built channel columns with lacing and lengths of 30 to 40 ft. Moreover, even if a very large testing machine cannot be installed, columns with compound web, like those of the Blackwell's Island Bridge, New York City, or of the ill-fated Quebec structure, but of smaller proportions, should be tested with different weights

Mr. Derleth. and stiffness of lacing. The capacity of the machine now available at the Watertown Arsenal is entirely inadequate for this purpose.

Since the report of the Committee does not give promise of an early building of a machine of great capacity the following remarks are restricted to the possibilities of the present equipment and to a discussion of the tests as outlined.

A large number of tests should be made, but the specimens tested should be restricted to a few standard shapes as recommended by the Committee. The variations should be in grades of metal and particularly in types of details. A few tests for each type of main section that has been in use during the past decade would only multiply results and confuse the conclusions. Columns in mild steel (60,000 lbs. strength), medium steel (65,000 to 70,000 lbs.) and nickel steel (90,000 lbs.) should be built with identical sections and details.

Any one group of members should show much variation in the details proposed. In the sketches of end connections, lacing, tie plates and riveting the committee has made an excellent start. Column splices for buildings, bolted connections, batten, lacing and riveting should be varied so that the tests of any one group may bring out clearly the effects of a range of proportions in these details. The effect of rivet holes (punched, reamed, or drilled) is worthy of careful examination. Results from such studies would be very valuable to the structural engineer for steel frame buildings.

The proposed tests contemplate the use of pins in all cases parallel to the web. Why has the program omitted some specimens of columns in which the pins are at right angles to the web?

In my judgment it would add to the value of the tests if the program included tests of cast steel struts as well as cylindric welded tubing. Cast steel struts of stout H sections, 10 to 15 ft. long, with pin ends, are now being used to an increasing extent in bridges.

It would also be advantageous to include struts of four angles, with corners out, and lacing on four sides.

The details for each type of section should be varied to the fullest extent permissible, limiting the variations only by the extent of the appropriation for defraying expenses. If no larger testing machine becomes available, then the built I section will lend itself readily to tests with large values of l/r in one direction and small values in the other. What is desired however is a test of a com-

pound of two such sections held apart by light or weak lacing. **Mr. Derleth.** Each group should include column specimens otherwise identical, but with the different conditions of end as outlined by the committee.

Between the extremes of the reinforced concrete column, and the unprotected structural steel column or bridge post, stands the column of rolled steel encased in a fireproofing of concrete, the enclosing concrete itself being usually reinforced with wire in the form of wrapping around the structural steel member. This type is common in first-class steel-frame buildings. In its design conservative practice makes no allowance for the concrete in determining the supporting capacity of the post. Tests to throw light upon this case are much needed.

In the present state of our knowledge of the capacity of reinforced concrete columns strengthened with longitudinal rods and hooping, it is not proper to place great reliance upon the hooping steel. Tests show that the hooping steel greatly increases the toughness of the column, that is, permits of relatively large ultimate strengths with considerable deflection, when the test is compared with that of a column of equal size of unreinforced concrete. But within working conditions, that is, with loads producing safe concrete stress intensities and safe steel deformations within the elastic limit, the economy of hooping is not great. Moreover, longitudinal steel in concrete columns is far from economic. With a working stress of 500 lbs. per square inch allowed in the concrete, 16,000 lbs. per square inch in the longitudinal steel rods and a ratio of 15 for the coefficients of elasticity, it can be shown that there is no economy in the use of more than 4 per cent. of steel; indeed that there is no justification in the use of reinforced columns with larger amounts of metal. Columns with larger percentages of steel cannot compare favorably in cost, size or stiffness with all-steel columns encased in fireproofing. Large buildings of reinforced concrete, I believe, will soon be built with structural steel columns in the lower stories. Where reinforced concrete has been used for columns in the lower stories of high buildings in California it has, in some instances that have come to my attention, been admitted by the designers that steel columns would have been cheaper, but that they wished the structure to be entirely of reinforced concrete.

The columns of the McGraw building for the Engineering

Mr. Derleth. Record in New York City show the tendency to use structural steel in part reinforced by concrete, or vice versa, as distinguished from the true reinforced column with low percentages of metal. With a testing machine of 800,000 lbs. capacity and 26 ft. length it would be possible to test 6- or 8-in. channel columns enclosed in a wrapping of wire and concrete, using general dimensions and details to be found in every-day building construction for small posts. Recently the writer had called to his attention columns of questioned strength in a building in the City of Oakland, California. These columns had an effective length of 11 ft. and consisted of 2–6-in. 8-lb. channels and 2–8 x $\frac{5}{16}$-in. cover plates. Neglecting the strengthening effect of the concrete fireproofing and the effect of end constraint due to the riveted connection of the floors, the formulas of the New York building laws allow about 120,000 lbs. safe calculated load. The actual computed weights of the building resting upon these columns at least equaled this amount, and perhaps exceeded it by 5 per cent. The columns were criticized by a Committee and it was recommended that they be reinforced. The writer does not advocate weak construction, but here is a case in point where definite knowledge as to the strength of such columns would have been of great value. Architects are constantly called upon to plan buildings for limited expenditures, and columns like other parts of the building are often skimped to extremes.

I recommend that the program of tests include the two following cases:

I. Six and eight-inch channel columns (a) with lacing (b) with cover plates, fireproofed with concrete reinforced with wrappings, such as Clinton wire mesh or the wire fabric of the American Steel and Wire Company, to determine the effect of the concrete on the strength of the composite column. For comparisons the steel work of these columns could be identical with purely structural steel specimens already included in the Committee's program.

II. Columns of structural steel (four angles and lacing) enclosing a central concrete mass, the compound member itself being fireproofed with concrete; the designs to be similar in principle to the columns of the McGraw building.

For structural engineers concerned with the design of buildings, tests of columns with large eccentric loads are advised; for

example, tests copying conditions of columns carrying eccentric cornice or balcony weights or stressed by mezzanine cantilevers supported half way up the column's length. **Mr. Derleth.**

Mr. Albert I. Frye (by letter).—Without regard to order of importance, tests of structural material on such a comprehensive plan as outlined by the Committee should have three main objects in view, namely, (1) to secure reliable data for the construction of practical column formulas; (2) to record the action and resistance of full-size columns to serve as guides in proportioning or designing the principal standard shapes and built sections until reliable formulas can be framed; and (3) to study the effects of variations in design and details with a view to economy or perfection. These will be discussed in the order mentioned. **Mr. Frye.**

(1) *Data for the Construction of Column Formulas.*—Of course all data will be more or less valuable for this purpose but certain simple shapes and built sections should be selected with the special view of determining the relation between the allowable load per square inch p, the radius of gyration r (or some power of r, as r^2), and the length of column l (or some power of l, as l^2), for various end and *middle* conditions of columns. In this connection the following additional clause is proposed:

15a. *Middle Conditions.* The "standard tests" to be made for two conditions of middles or centers, for the higher values of l/r: (a) centers free; (b) centers fixed in direction of least radius of gyration. [Compare with Clause 15.]

Outside of the theoretical value of these data, the practical value will appeal to many. Most engineers who have had to do with the design of trusses whose web members are stiffened at some central point, have been at a loss to decide on the "length" of column to use in connection with the corresponding effective radius of gyration. Designers of Howe trusses variously use one-half, five-eighths and three-fourths of the total length of the intersecting brace as the effective length in proportioning them. We meet with the same problem in the sub-panel truss, where the vertical posts are stiffened laterally by horizontal struts or rods connecting them near their middle and lying in the plane of the truss and also in many Pratt trusses, both steel and combination. It is suggested that I-beam and H-sections be included in these

Mr. Frye. tests, using the same sections, lengths and end conditions as those which are tested under Clause 15, so that the direct effect of lateral stiffness can be determined by comparison, and reduced to allowable "length" of column where lateral support is given.

Regarding end conditions, it is suggested that where pin-ends are used the pins be placed both in the plane of the least, as well as the greatest, radius of gyration, in a number of cases.

(2) *Action and Resistance of Full-Sized Columns.*—It is suggested that photographs be taken of the columns at various critical stages of the tests, as when defect is shown in riveting, or plates buckle, or a marked permanent set appears, and always after the test has been completed to destruction. In this connection the writer begs to differ somewhat from the conclusions drawn in Clause 20, and would urge that the sections for the present be limited in size, and that the end conditions be such that they can be maintained throughout the test, in order that every column as far as practicable may be tested to failure. Substituting pin-ends for flat-ends in the midst of a test would hardly give reliable data for a pin-end condition maintained constantly from the beginning; and anything short of absolute failure would leave a gap or incomplete record. A testing machine of greater capacity will doubtless be installed at an early date for testing large sections.

(3) *Variations in Designs and Details.*—This is a most important subject and should receive careful consideration. Many of the points are covered in Clause 17 of the Preliminary Program, but there is one matter which the writer would like to bring to the attention of the Committee, namely, the treatment in an auxiliary way of the thin webs or plates of a column so that they may become in fact, as in theory, integral factors in the column's strength, as indicated by the radius of gyration. It is here suggested that columns with plates or webs that are very thin in proportion to their width or depth should have these thin members supported at two points in their length by steel diaphragms, each one spaced about one-fourth to one-third the length of column from the end, and riveted transversely to all sectional members. A few tests could be made on reduced specimens, enough to guide the Committee in framing a rule or formula for their introduction, or at least in making recommendations for their use in special cases. And this brings up another matter which the writer has had

under consideration for a long time, namely, tests with reduced specimens. **Mr. Frye.**

Tests with Reduced Specimens.—Structures of large size are now being built and will continue to be built, the individual members of which will be beyond the capacity of the testing machine, or at least of such large proportions that the expense of testing such members would be enormous. Now if we could test reduced specimens in such cases in place of the full-size members, and at the same time get a just or fair estimate of the strength of the latter, it would mean a great saving in cost. Indeed, members of large section could probably be designed intelligently with the use of proposed and existing data on tests of smaller or usual sections.

To illustrate, let us assume a steel tube of 24 ins. internal diameter, 240 ins. long, with metal 1 in. thick. Then $l = 240$, $r = 8.84$ and $l/r = 27.14$. Now let us assume another steel tube just one-half the size proportionately by scale, that is, 12 ins. internal diameter and 120 ins. long, with metal $\frac{1}{2}$ in. thick. Then $l = 120$, $r = 4.42$ and $l/r = 27.14$. Note that the value of l/r is the same in both cases; in fact, the value of l/r will remain constant for any proportionate reduction or enlargement of scale. This is true of any section and is a very important fact in connection with the matter under consideration. We hear it stated frequently that tests of small specimens will not give reliable data for full-size columns. This is the point which the writer would like to bring out. No column formula in use, with which the writer is acquainted, recognizes this as being true. In the two examples cited above, the allowable load per square inch of column section would remain constant, and continue constant for any change of scale. Having this in mind it is here suggested that columns of various standard sections be designed, and that actual columns be built after these designs, but using different scales, so that all parts, including length and riveting, be increased or decreased proportionately and placed under test. It can then be demonstrated what factor in addition to l/r will have to be considered in designing columns of large size. A sufficient number of tests of graded sizes from the same sections should be made so that a curve showing the value of p, the allowable load per square inch, can be drawn and produced for very large columns of the standard sections used.

In conclusion, the writer wishes to express himself as heartily

Mr. Frye. in favor of the program submitted by the Committee, in a general way, and ventures to predict that contingencies will likely arise during the progress of the tests which will necessitate changing the program to a greater or less extent. For this reason, careful study should be unceasing throughout the progress of the work in order to take advantage of necessary or desirable changes from any hide-bound program.

Mr. Gifford. MR. G. E. GIFFORD (by letter).—I have been glad to look through this program, which in general seems to be very well planned. There seems, however, to be too little attention paid to the forms of columns used in building construction, and too much attention to the column forms for bridge construction. The structural business for buildings is daily becoming more important and the knowledge of structural designing has scarcely kept pace with this development nor with the designing of bridges. Furthermore, the quality of material to be used for these tests is not the common standard for structural steel for building work. The latter is usually made after the manufacturers' standard specifications, or their equivalent, and it would seem to me proper to make some tests on material of this grade, as well as on that made after the proposed specifications, and on "medium steel" which is usually the material for building work, rather than "bridge steel" which is not commonly specified.

I am pleased to submit herewith some observations by our Mr. T. J. Bird, Chief of the Designing Department, which pertain to the subject in hand:

The preliminary program does not include one of the most common forms of column used in buildings, i. e., the two-channel and two-plate column, nor does it cover columns of continuous lengths. The latter occur not only in building work where columns run through from story to story, and are only partially held at the floor levels; but also in bridges where truss chords are imperfectly held at the panel points.

I would recommend the investigation of another law for columns than that of the ratio l/r. This I think will be found in the relation of the thickness of material to the projection from the neutral axis. Thus, a 3 x 3 x $\frac{1}{4}$-in. angle has a larger radius of gyration than a 3 x 3 x $\frac{1}{2}$-in. angle, but the latter will undoubtedly carry more than twice as great a load. This is a matter I have had in mind for some time, and had intended making some tests in our shop, but time did not permit. I refer to this especially because the schedule includes a number of sections composed of angles, as well as wide-flange H sections, and I feel satisfied that l/r is not the only feature determining the strength of such sections.

In the description of the material described, no reference is made to the chemical analysis of test pieces. I think this is quite important for material that is to be used in compression, because the percentage of carbon influences the compressive strength considerably. **Mr. Gifford.**

In noting stresses at different stages of deformation, some method should be used for ascertaining the instant the column shaft begins to bulge, because additional bending stresses are then developed. From tests at our own shops, we have found that the ultimate strength is no guide to the carrying capacity of columns. We have had failures take place immediately prior to which there was no sign of deformation of the members; again we have had failures occur almost immediately after the first signs of deformation in the members; and then again we have had members show considerable deformation before failure.

(The tests here referred to were on certain complete structures where the object was rather the ascertainment of the strength of the structure as a whole, than observation of minor points of failure.)

MR. THOMAS H. JOHNSON (by letter).—I have read, with much interest, the proposed schedule of tests of compression members. I understand that the several designs of columns shown in Figs. 1 to 8 inclusive, are for a preliminary series of tests, and, presumably, will be followed later by other designs. **Mr. Johnson.**

With reference to the interpretation of results, I wish to suggest to those having that part of the work in charge, the importance of making a broad distinction between those members which fail by local crippling and those which fail by deflection as a whole.

The latter only should be used in checking existing column formulas or deducing new ones. The former will be serviceable in determining the extent to which the approved formula must be modified for those forms of section which show a tendency to local crippling, a weakness which will always prevail in sections with wide horizontal flanges unsupported.

I anticipate that the built I section shown in Fig. 3, of the program will be most likely to show this type of local failure. The rolled section, Fig. 2, will be less apt to develop this form of failure because the varying thickness of the flanges will give them greater stiffness. In view of the wide-flanged I beams now being rolled at some of the mills, it will be interesting to note the behavior of Fig. 2 in the report of these tests.

The double channel sections if made of rolled channels are not so apt to fail locally as those built of plates and angles,

Mr. Johnson. especially when the free leg is relatively wide. You do not show this built form in your scheme of preliminary tests, but it will doubtless be found in other schemes to follow. When you reach this, I would suggest that a series of tests be made with different distances between hitches of lacing bars, with a view to determining the maximum spacing allowable without local failure.

Also, I would suggest that your Fig. 3 section, and the built double channel sections just referred to, be duplicated with "bulb" angles which will afford some stiffness to the outer edge of the flange. I think these would show better results than the plain angles; and still better results if a channel with one short leg, instead of the bulb, were available, thus giving greater stiffness to the outer edge of the flange.

The superior behavior of the Z bar columns in Mr. Buchanan's tests is evidently due to the support of the horizontal leg afforded by the outer leg of the Z. My idea is that if this outer leg were turned in the opposite direction the section as a whole would afford greater freedom of detail in the attachments, and that this leg, serving merely as a stiffening rib to the outer edge of the flange, need not be as deep as the opposite leg which takes the rivets. This form has been used in certain cases of large built I sections in which the flanges were made of plates and angles, stiffening angles being riveted along the outer edge of the plate; but I think it would be desirable to have the same effect in a rolled form applicable to smaller sections. At present the "bulb" angle is the only form available which approximates this condition.

In all cases of latticed columns, the recorded data should include the determination of the value of l/r for the individual members of length between hitches of lacing bars, and its comparison with the corresponding value for the column as a whole.

Also, it will be interesting and instructive to have some tests of columns in which the lacing bars are replaced by batten plates, wide enough to take 3 or 4 rivets, and spaced so that the value of l/r for the free portion of the individual member shall be equated with that of the whole column, treating the individual member as a fixed end column. In the latticed type, with riveted hitch, this individual member must be considered as having hinged ends; the actual form of end bearing of the whole column should be recognized in equating with this detail.

MR. BERNARD KIRSCH (*Vienna*, by letter).—The program of the tests outlined by the Committee is very satisfactory and its execution will clear up many doubtful points. As it refers mostly to riveted columns, I think that the following points should be carefully observed: **Mr. Kirsch.**

1. The great influence of even small eccentricities (a few millimeters) and the circumstance that through imperfect workmanship and erection much greater eccentricities are unavoidable makes it desirable to make tests with eccentricities of about five to ten millimeters ($\frac{3}{16}$ to $\frac{3}{8}$ in.). This is, of course, only possible by using special methods of transmission of the pressure at the ends of the columns.

In riveted connections, it is possible that deformations take place before the theoretical load is reached (a sort of lost motion in the rivets) which would also cause eccentricities, making tests with small eccentricities of great value.

2. My investigations have convinced me that buckling begins exceedingly slowly. Theoretically, in case of axial loading the unstable equilibrium is maintained for some time until a vibration caused by the loading, which is nothing else but the appearance of an eccentricity, causes a sudden buckling of the column, representing the second (stable) equilibrium.

In practice this seldom happens. As a rule, there is a small initial eccentricity which starts the buckling very slowly when the pressure at the extreme fiber reaches the limit. This buckling may amount to only a few hundredths of a millimeter and may last for hours. If the load is increased with the same speed as at the beginning of the test, a greater buckling load will be found. For this reason, almost all buckling loads found in older tests are about 10 per cent. too great. I would, therefore, suggest to proceed very slowly in the last phase of the test in order to establish the true buckling strength.

MR. GUSTAV LINDENTHAL (by letter).—It seems to me that the program of tests, if carried out, will not add much of value to our present knowledge on compression members, scanty as that is. **Mr. Lindenthal.**

Tests on large compression members are needed more than anything else. To that end, it would have been wiser to wait and save the money that the proposed tests at Watertown Arsenal will cost, and endeavor to obtain additional appropriations or voluntary

Mr. Lindenthal. contributions, until a sufficient amount is on hand to install a testing plant on the lines of the enclosed sketch (Fig. 1) with a capacity of 50 million pounds pressure. Such a plant, including the building, will not be as expensive as some of the existing plants for the testing of large tension members, of which we have already ample information.

The so-called straight-line formula for columns has been accepted in good practice with good reason as the most convenient, and as fully reliable for such sections as have been tested, when l/r is limited to 100. No pressing need, therefore, exists for tests to find another formula giving perhaps closer agreement with theory. But a grave question has arisen, particularly in connection with large compression members, as to whether different types, some of which have never been tested, really have the same value

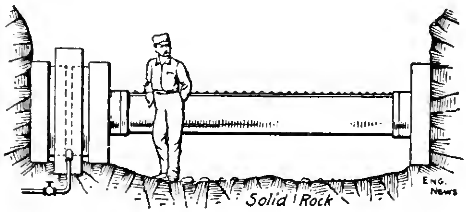


FIG. 1.

when the sectional area, the length, and the radius of gyration are the same. We are totally ignorant of the actual effect of the shearing stresses set up under compression, in different types of columns.

All compression formulas agree in making the value of the column dependent upon the relation l/r which is based solely upon bending moments, without regard to shear. And it is, at present, assumed that columns of different types, but having otherwise the same conditions of bearings, the same area of cross section, the same length, and the same radius of gyration, have also the same strength. For this assumption, universally accepted, we have, however, no proof from any tests yet made. This proof we should have before scientific progress can be made.

Paragaph 19 of the program specifies four different types, but does not specify that the types must have the same l/r and the same sectional area so as to be comparable.

Mr. Lindenthal.

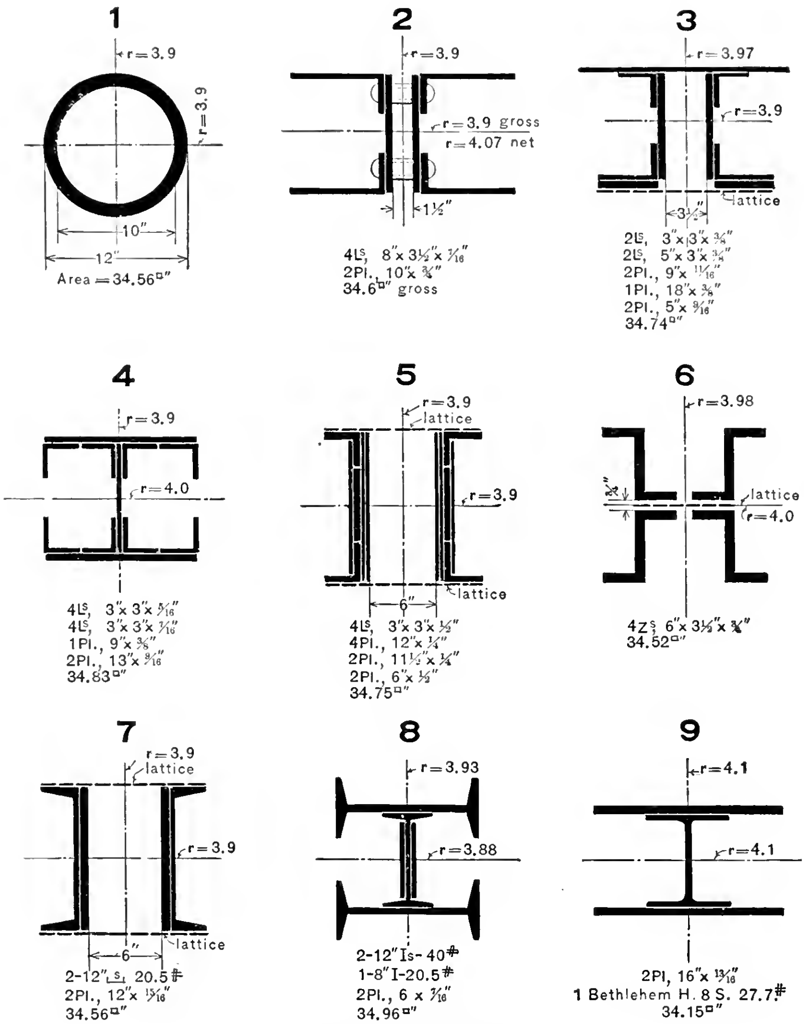
The program covers a multitude of other tests that seem to be irrelevant and unnecessary. What necessity exists, for instance, for testing columns having "eccentricity of loading due to shifting of pins," or other avoidable eccentricity of loading, or "columns lightly spliced at center with imperfect end contact," or to determine the effect of "tight and loose riveting," or the efficiency of "intermediate batten plates?" All these conditions are recognized and condemned as bad practice. We surely have no need of tests to establish the degree of badness in poor design or workmanship. We might as well test eye bars with pin holes out of center, or with heads thinner than the shank, or other preventable bad conditions. What is wanted is knowledge of the behavior of columns built in conformity with any good bridge or structural specification in use, and of uniform quality of steel and workmanship.

The supplementary program still further enlarges the unnecessary heterogeneity of the tests.

Assuming that all the tests of the program as proposed are made, what useful knowledge will they furnish that we do not already have, little as that is?

I am of the opinion that the program should be for a series of tests on groups of columns of different types, which tests should precede those with columns of very large sections, which are prohibitively expensive. Each group should comprise three columns of a single type and of the same size so as to get comparable values from each group and type. The first group should be of the annular type and all following groups or types should have the same l/r and the same cross section. Two columns of each group should be pin bearing at both ends, but the pins in the second column (except for the annular type) should be at right angles to the pins in the first column; the third column should be square-bearing at both ends.

The types should be those most prevailing in structural construction. The accompanying sketches (Fig. 2) illustrate my idea as to sections. All the nine different types have approximately the same value of l/r on two axes at right angles to each other, and the same cross section. Such tests would show whether the different types have really the same buckling strength or not.



Column Cross Sections.

All having the radii of gyration, (at right angles to each other) approximately = 3.9" and areas, approximately = 34.6□"

FIG. 2.

Columns of large section should be tested, whenever there is an opportunity to do so, to verify the conclusions from the smaller columns. More than one large testing plant would not be needed in this country, but its need is paramount. **Mr. Lindenthal.**

At present, we are unable to ascertain from computation the buckling strength of some types that have never been tested within 50 per cent. of their actual values. This is certainly not creditable to engineering as a science. Even carpenters can guess the strength of posts in their work closer than that.

MR. ALBERT LUCIUS (by letter).—I beg to submit that it would be most desirable to ascertain with certainty whether it is true, as previous tests seem to show, that the elastic deformation of steel in tension and compression is a constant factor and a direct ratio of the unit stress for all steels, soft, medium, or high grade, irrespective of their elastic limit, or their ultimate strength, and independent also of their chemical constitution within the range of application for structural purposes, and including nickel and chrome steel alloys the same as merely carbon-manganese alloys in this remarkable property, namely, that the elastic deformation is not a function of the strength, and not a function of the resiliency, but a simple constant and uniform ratio of the unit stress for all structural steels alike. **Mr. Lucius.**

The bearing which this question has on the desirability of using high ultimate steels for bridges in the future is apparent and of the greatest importance.

For what would be gained by using high-grade material at high-unit stresses if this would increase the deflections and vibrations in the same ratio as the unit stresses are increased and if the percentage of impact would therefore have to be increased, and added to the total sum of stresses, not only in the same ratio as the vibrations increase but further on account of the relatively greater amount of live load to dead load which would result from the use of high unit stresses in bridge structures?

I respectfully submit, therefore, that it is desirable to make conclusive tests establishing the facts in connection with the elastic deformation of various steel alloys of various compositions, and various elastic limits and ultimate strengths within the limits of their practical use for structural purposes.

Mr. Merriman.

MR. MANSFIELD MERRIMAN.—The recent investigations and tests made by Professor Lilly of Trinity College, Dublin,* indicate that other elements of the cross-section besides area and radius of gyration influence the strength of a column. He has shown that the unit-loads causing the failure of two columns of the same material are not the same for the same slenderness ratio l/r when the cross-sections are arranged in different ways. As the simplest case, take two hollow circular columns having the same radius of gyration and the same length, but of different sectional areas, then the unit-load P/A is found to be the least for the column of least sectional area. Also for columns composed of plates or webs he finds that the thickness of the plates or webs, as well as the radius of gyration of the section, exercises an influence upon the strength. If these deductions of Professor Lilly are correct, and they seem so to the writer, it follows that the column formulas now in use are not applicable to built-up sections except in so far as they represent the results of tests. This empirical representation, moreover, is only a rough average for the tests heretofore made, and there is no assurance that it applies even approximately to columns of unusual sections.

The program of tests seems well arranged to throw light upon this important matter, since the first and third sections of Material Schedule No. 1 have the same radius of gyration. Perhaps also it may be possible to obtain for the second section an H-bar having the same radius of gyration as the first and third sections. Although the fourth section of the schedule has a larger radius of gyration, the program of testing all columns for certain values of l/r is one well adapted to ascertain in a general way the influence of other elements. Unfortunately the capacity of the machine seems to forbid the use of other sections which might give more decisive information than can be obtained from those proposed.

With regard to the special tests, the writer suggests that it might be advantageous to take measurements of the lateral spreading of the channels of the fourth section, since, if such

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* The Strength of Columns," by W. E. Lilly. Minutes of Proceedings of the Institution of Civil Engineers, June, 1905, pp. 697–722.

"The Economic Design of Columns," by W. E. Lilly. Institution of Civil Engineers of Ireland, March, 1906, 26 pages and 4 plates.

"The Quebec Bridge Disaster," by W. E. Lilly. Institution of Civil Engineers of Ireland, December, 1907, 17 pages and 7 plates.

occurs, the stresses in the lacing or latticing are reduced. The program seems to be carefully and admirably arranged throughout, and it is to be hoped that the proposed comprehensive series of tests may soon be made. Mr. Merriman.

MR. C. M. MILLS (by letter).—The tests, in view of the observations expected to be made with the compressometer, the details to be recorded, and the uniformity in the source of the material and of the workmanship on the specimens, promise information now lacking. Mr. Mills.

The specimens are well selected considering the restricted capacity of the testing machine and cover many of the questions of column and strut design requiring experimental bases, the sizes being sufficiently large to furnish valuable information.

Tests of annular sections promise valuable data by which to coördinate other sections, and it is hoped that their range will ultimately be extended, with correlated specimens of other types.

It is supposed that the tests to be made on latticing will include various proportions of latticing, irrespective of the batten plates. Since the latticing shown on the drawings of the specimens is relatively heavy, variations in this respect seem desirable.

It is noticed by the schedules that the specimens to be tested with pin ends are to have the pins parallel to the webs, where this particular is stated. Since in practice the position of the pin is more frequently perpendicular to the web, it is desirable to have tests made under this condition.

It is supposed that the additional special tests contemplated will include pin-end specimens with forked ends, as experimental information covering such details is important.

It is also desirable that among the special tests contemplated, cases of extraneous lateral loading in combination with axial loading be included, since this duty is frequently required in compression numbers. It is appreciated that well-established data covering axial loading will have importance in treating such cases.

The future tests should also include those covering the requisite thickness of unsupported width of cover and web plates for axial loading.

It is evident that deviations from a preliminary program will be found necessary as the work progresses. Careful scrutiny of the current results will be required to indicate the most fruitful course

Mr. Mills. to follow in the further selection of specimens, and the mode of testing and observation.

Mr. Osborn. MR. F. C. OSBORN (by letter).—In reference to these tests, the writer would like to call attention to the extreme importance of careful measurements and observations previous to the reaching of the elastic limit of the materials. In a great majority of the column tests heretofore made, practically no information has been obtained except that relating to ultimate failure. If we are to have any guide for a proper column formula, it would seem that the data should be based on information obtained within the elastic limit of the material. It is within the elastic limit that we intend to work practically, and it is only within the elastic limit that the action of steel is reliable or regular.

The program indicates in paragraph 18 that it is the intention to take careful note of measurements of elongation, compression and deformation, and the point I wish to emphasize is that the greatest care should be given to these measurements at various periods preceding elastic limit.

The series of tests outlined should be productive of much valuable information, but it seems that ultimate failure is a very small part of the information actually desired.

Mr. Prichard. MR. HENRY S. PRICHARD (by letter).—The Committee have arranged a good program for the tests of steel columns. The previous experiments have been few and engineers have had to rely chiefly on tests of wrought iron columns for their experimental data.

The tests of iron and steel columns previously made show conclusively that centrally loaded columns, as constructed and used in practice, do not follow any simple law. A simple law by which the strength of seemingly centrally loaded columns could be foretold with as much reliability as, for instance, the strength of eye bars, would be a boon, but it is useless to hope for its discovery.

The determination of the strength and behavior of columns is in fact a very complicated problem depending on a great variety of conditions which cannot be very closely determined and are liable to vary in practice. To some extent the strength and behavior within the elastic limit of columns under various conditions are susceptible of analysis; Prof. Cain's analysis of the ideal column

Mr. Prichard.

centrally loaded,* Prof. Marston's analysis of the ideal eccentrically loaded column,† Prof. Fidler's analysis of the effect on columns of variation in the modulus of elasticity‡, and similar analyses by others, appear to be substantially sound, and, although they leave many problems unsolved, should, if proven, be of great assistance in interpreting tests and in deciding on simple rules for practice. Engineers, however, quite naturally decline to fully accept them before they have been verified by experiments.

The tests of eccentrically loaded columns forecast in the program will doubtless afford opportunity for comparisons with theory, but the number of cases is limited and the comparison in some cases is complicated by friction on pins.

The value of the tests will be so enhanced if they establish a reliable theory by which their results can have a wide range of application, that the following suggestion is made: *Provide removable hardened steel caps for columns with flat ends, so arranged that the loads can be applied with various degrees of eccentricity by means of hardened steel edges.* By this arrangement the position of the real axis of the column could be found, numerous tests of eccentric loading well within the elastic limit could be made, and the deflections obtained for comparison with theory, before the columns were finally tested to destruction with flat ends according to program.

For columns of any considerable length, those with pin ends show much higher results in tests than those with round or pivoted ends, but it is doubtful whether this additional strength can be relied on in practice. From theoretical considerations it seems highly probable that a little eccentricity in the application of the load will overcome the tendency of the friction on the pins to fix the ends; in other words, friction on pins gives columns with such connections a seeming strength which is probably delusive and which when most needed might not exist. If some of the columns to be tested with eccentrically placed pins could be first tested well within the elastic limit with some arrangement of knife edges having the same eccentricity or, by means of bushings, with smaller oiled pins, the deflections would afford a comparison which would

* "Trans. Am. Soc. C. E.," Vol. XXXIX, pp. 96–107.

† "Trans. Am. Soc. C. E.," Vol. XXXIX, pp. 108–113.

‡ "Proceedings of the Institution of Civil Engineers, Vol. LXXXVI, Part IV.

Mr. Prichard. to some extent indicate the value of friction on pins in stiffening the ends.

The program provides for tests to determine the moduli of elasticity in tension and compression. Would it not be well to make this a very prominent feature? On their face existing experiments indicate a great range for the modulus of elasticity of steel. In Mr. Christie's experiments, values as low as 15,000,000 lbs. per square inch were obtained* and in numerous tests at the Watertown Arsenal extensions are recorded corresponding to moduli of from 80,000,000 to 120,000,000 lbs. per square inch. Of course these large values must be in error, but there is some variation, and its reasonable limits have not been determined. Its importance can be realized by contemplating what would happen in a latticed channel column in which one channel had a modulus of 30,000,000 and the other 15,000,000 lbs. per square inch. It has been claimed, and it is probably true, that the real modulus is that which is observed on the second or subsequent application of the stress, and that its range is small; but in the case of columns, deformation depends on the shortening which takes place on the first application of the load and the subsequent improvement in elasticity is at the expense of a detrimental permanent set. Experiments on the modulus of elasticity to be of much value in removing the doubt which exists regarding it should be quite numerous, cover the entire range of stress within the elastic limit, and should be made from numerous heats.

Slight imperfections in elasticity are liable to occur under quite small loads, and they naturally create doubt as to the strength of columns. Would it not be well to stop the tests of some columns when the loads are fairly high but before serious deformation has occurred and subsequently retest them after a lapse of some days, so as to determine whether or not the slight imperfections in elasticity occur only on the first application of the load?

The program provides for a few tests of channel columns with 12 x $\frac{1}{4}$-in., 14 x $\frac{1}{4}$-in., and 16 x $\frac{1}{4}$-in. cover plates. Would it not be well to considerably enlarge this feature of the program? The determination of the influence on the strength of columns of the ratio of unsupported width to thickness of plates is an important

* "Trans. Am. Soc. C. E.," Vol. XIII, pp. 253–285.

matter regarding which information can be obtained only by experiments. It is, therefore, suggested that the number of tests should be considerably increased and the range of ratio of unsupported width to thickness enlarged. **Mr. Prichard.**

It is also suggested that tests should be made of channel columns with a number of thin cover plates riveted together by small rivets, so as to determine the relative resistance to crumpling of built up and solid cover plates. The large compression members used in practice of necessity have their webs and covers composed of a number of plates.

Experiments might also be made to determine the proper spacing of the rivets connecting the covers to the channels.

The importance of good rules for designing lattice bars is now generally recognized. In addition to the tests of latticing outlined in the program, would it not be well to construct some lattice columns with very small angles for lattice, connected by small rivets, arranging some of the columns so that the intersections of the lattice would occur on the centers of gravity of the channels, using connection plates similarly as for lattice girders, and arranging others so that the connections would be eccentric? In this connection some of the columns could be loaded eccentrically. The information obtained from such tests would be more useful in the design of very large compression members than the results of tests in which the lattice bars were of the usual size and type for channel columns.

The opportunity afforded by the experiments to establish the fundamental principles governing the design of steel columns should be utilized to the maximum extent, as the tests can be of more value in this regard than as direct criteria for design.

MR. H. H. QUIMBY (by letter).—The element of time in the application of the load should receive attention in the test of both long and short members. Recent tests of compressive resistance to loads continued for hours and for days developed unexpectedly low values—both elastic and ultimate—and indicate the importance of extremely slow speed of loading in making tests. In such structures as cantilever and bascule bridge trusses, and columns under walls, where the dead load which is continual constitutes the larger portion of the stress, regard should be had for the continuance of the stress. Because of the fact of the actual flow of the **Mr. Quimby.**

Mr. Quimby. metal, slow as it is, a member may sustain for an hour or a day what it will not for a week or a month. The point of actual flow of metal under long duration of loading is the vital point for members in which the continuing dead load stress is the principal one, and as the ultimate unit resistance of a full size member is not that of a specimen but depends in large measure upon the shape of the member, i.e., the distribution of the metal, as well as upon its ratio of length to radius of gyration, so the real yield point, or the elastic limit of a column, may be much less than that of a specimen.

The new heavy H sections of the Bethlehem Steel Company seem to be most promising for adaptability, economy and neatness of design, and because of their newness and the total lack of data respecting the resisting value of just such a shape, it is to be hoped that the tests of the largest sizes that are within the capacity of the machine will be given precedence over the tests of the other forms which are better known.

Mr. Ricketts. MR. PALMER C. RICKETTS (by letter).—The personnel of the Committee on Program of Tests insured an intelligent program. Its recommendations have been very wise and satisfactory. The supplementary program is, in my opinion, the more important one, in as far as it relates to column tests. The higher range of tensile strength for the carbon steel in the supplementary program insures, I believe, results of more practical value than if the superior limit were maintained at 65,000 pounds, as suggested in the preliminary program. The nickel steel tests will also be of special value. The effect of workmanship for this material cannot be too carefully investigated.

The recommendations of the Committee so closely coincide with my views that I should not have written these few words if I had not believed it to be the duty of every engineer to second the efforts of those who have urged the construction of a very large testing machine at Watertown Arsenal. Such a machine cannot well be constructed by private effort. The United States Government should supplement the very valuable work it has been doing for so many years at Watertown by experiments upon columns of a larger size; of sufficient size to render the results valuable in the design of the compression members of great modern bridges. I believe that if a concerted effort, inaugurated by the American Society for Testing Materials, were made by the great engineering

societies, a sufficient appropriation for such a machine could be obtained from Congress. **Mr. Ricketts.**

MR. F. SCHÜLE (*Zürich*, by letter).—It is extremely gratifying that by extensive experiments on compression members a thorough foundation for the arrangement and proportioning of such parts will be obtained to which no objections can be raised. **Mr. Schüle.**

The program established by the Committee with such great care covers the cross-sections and the general design of compression members most frequently used. In order to be able to review the results more clearly, it is quite desirable to limit the tests to a few sections. Three points, however, I would like to submit to the Committee for consideration.

First.—The selected sections all have their centers of gravity in the center, that is, they are symmetrical to both main axes. Not infrequently it is necessary, however, on account of the construction, to design a compression member which is symmetrical for one main axis only, as for example, a built I section with a cover plate on one flange only. How would such sections act under a centrally applied load? I think it very desirable to settle this question by experiments.

Second.—The tests are intended to be carried out on each column with mild steel, which, if I understand the program correctly, will be rolled from the same heat. For shorter columns where the yield point will very probably be reached in the compression tests, it seems quite important to establish whether the limits of the tensile strength permissible under the specifications would not result in a premature destruction, if in the same column section, one part should be made of steel of the maximum permissible stress and another part of steel of the minimum permissible stress. The proportion of the tensile strength (55,000 to 65,000 lbs.) is liable to be still more pronounced in the yield point, depending upon the rolling temperature, and might have a bad influence on the effective strength of short columns.

Third.—In the lattice columns (double channel sections), the intended experiments on the effectiveness of the lacing should bring forward very necessary information. In accordance with the proportion of the rivet section to the net section of the lacing through the rivet holes (2.84 cm^2: 3.52 cm^2) it is to be expected that in most cases the rivets will be sheared off. Now the question

Mr. Schüle. of the shear for which such rivets should be proportioned is the very question which has not been cleared up for the design of such members. I raise the question whether the object of these tests would not be accomplished better if in columns, as here suggested, a first test would be made with rivets of only 10 or 12 mm. diameter. After these rivets have been sheared off, all the rivets of the columns should be removed and the holes reamed out to 14–16 mm. and new rivets inserted. After this, the second experiment could be made on the same column with rivets of 13 to 15 mm. diameter. After a number of these rivets had been sheared off, they could be removed again and the holes reamed out to 19 mm. and the third experiment made. In this way without the use of much more material very important results could be obtained.

Mr. Snow. MR. J. P. SNOW.—I fully concur with the program laid out and will not submit any discussion. The paragraph suggesting extensometer tests of various parts of the column, including lattice bars, flanges, etc., I deem most important. This matter should be fully gone into.

I hope the Committee will test a few columns with shorter tie plates than those shown in the drawings, say with three rivets or possibly two. I think it would be very interesting to some members of the profession if models of very heavy columns could be scientifically tested, although, perhaps, this is outside the function of the Committee.

Mr. Talbot. MR. A. N. TALBOT (by letter).—The proposed series of tests on steel columns is an undertaking of considerable magnitude, far beyond anything yet attempted. The results ought to be of great value to the engineering profession. It seems evident that the tests, as outlined, will give results of value in determining, for the size and make-up of the columns tested, ultimate strengths and methods of failure; and as the outline gives considerable variety and includes forms and dimensions used in ordinary structural work, the data will have important applications. Information about unusual sections like the circle (welded tube) and on new sections like the H-shape will also be welcome. The effect of variation in length will also be useful in making up formulas for columns of the kind tested.

The standard tests, may, then, be expected to give data of value in structural design, but it seems to me that in addition to

this such a series of tests should be arranged to secure information which will have a direct bearing upon the causes leading to incipient failure and which will assist in determining the actual distribution of stresses and the conditions which produce this distribution. In other words, emphasis should be placed on finding the cause of the low compressive stresses found in columns. As I look at it, the outline does not contemplate a sufficient or thorough study of the internal action of the column, of the bending, twisting, etc., or of the effect of slight defects in workmanship as is desirable. It goes without saying that while ultimate resisting strengths are important, we need information on internal action. We should have information that will enable us to remedy defects, to obviate large secondary stresses, and to formulate principles which may be used with confidence in the design of larger sizes or other forms of columns. Tests of steel columns made under my supervision during the past year show that unconsidered stresses exist and convince me of the importance of spending much thought and time in the study of these stresses. Doubtless the Committee contemplates work of this kind, farther than is expressed in the program, but it may be helpful to the Committee to have emphasis placed on this in this discussion, as it may serve to assist them in arranging for work of this character. Not knowing what the Committee had in mind in making up the program, it does not seem profitable to discuss the selection of columns made, though at long distance the writer does not fully agree with the selection. **Mr. Talbot.**

MR. S. T. WAGNER (by letter).—Since the receipt of a copy of the "Preliminary Program of Tests of Steel Columns," etc., I have given the subject some special study and have submitted it to two of my assistants who are specially engaged upon structural design. **Mr. Wagner.**

Every point that we have desired information upon seems to have been covered in the program submitted, even to some minor details that we did not expect to find.

It would be a waste of time for me to attempt any discussion of it from my point of view other than to say that it seems to me to cover the field very well and carefully, and that the order of procedure outside of the nature of the tests themselves seems to have been planned with great skill and foresight.

Mr. Walzel.

MR. A. WALZEL (*Brunn*, by letter).—Referring to the program for column tests at the Watertown Arsenal, the tests of columns consisting of two channels, latticed (Figs. 4 to 8), are evidently of prime importance, these being the typical compression members of American bridges.

In calculating these compression members, the moment of inertia about the center of gravity of the whole section is introduced in the compression formula assuming that the latticing is sufficiently strong to make the two parts act as a homogeneous whole. This uniform action depends principally on the kind and strength of the lattice members and their connections. For a system of latticing which does not guarantee a completely uniform action it is difficult to find a formula which expresses the proper influence of the moment of inertia.

All of the proposed tests have latticing consisting of flat bars which can easily buckle laterally, especially in the case of single latticing (Figs. 4 to 6), thus allowing the two halves of the column to act independently, that is, each with its own moment of inertia only.

I would suggest that tests be made also of columns latticed with stiff shapes, such as angles or channels. The shop work with such latticing is, in my opinion, not much more difficult and expensive, whereas I consider it much more effective, and am convinced that the tests will prove this. The kind of latticing should be as similar as possible to that of the flat bars, so as to have parallel tests.

The connecting rivets have also to be considered and I would recommend that tests be made with double shear connections with or without small connection plates.

Finally tests should be made with columns having on one side a full plate instead of the latticing, which, in my opinion, is still more effective.

I consider of smaller importance tests on I and star sections since these have already been cleared up by the late Professor Tetmajer.

I further recommend the measurement with electric contact screws and micrometers not only of the deflections in the center but also the deflections at or near the quarter points in order to get the shape of the elastic line of the deformed column.

MR. J. R. WORCESTER.—The outline of tests proposed by the Committee is in the main admirable, and the results of tests, if carried out exactly as suggested, would undoubtedly add information of vital importance to that available up to the present time. A careful reading of the preliminary report, however, suggests the criticism that the importance of eliminating variables in some of the series proposed has been lost sight of, thus lessening the value of the results and wasting good material. Having clearly in view the essential requirement of allowing but one variable at a time, the writer would offer the following suggestions. **Mr. Worcester.**

1. The effect of certain variables should be determined upon an ideal column, i. e., one of which the strength is dependent upon the whole area acting as a unit, and which fails as a whole, and not by the yielding of a detail. This column should be symmetrical and centrally loaded, and should be straight.

As to the form of this column, a hollow cylinder has been suggested. This would be advantageous in respect to absence of detail and distribution of metal, but should be excluded for two reasons: (*a*) because it is obsolete as a practical usable shape, and (*b*) because its manufacture involves processes other than those to which plain structural material is subjected.

A double channel and lattice column has the merit of being of a form very generally used. It is also formed of simple rolled sections. It is, however, objectionable because the details of the latticing form one of the elements of doubt which we require tests to determine.

An I or H section, rolled to the form, has many advantages, but is somewhat objectionable on account of uncertainty whether the metal receives the same work in rolling in all parts, and corresponds with the usual structural material in the form of plates and shapes.

On the whole, it seems as if the built-up I, consisting of 4 angles and a plate, possesses more of the desirable elements than either of the others proposed.

The variables which should be tried out on this ideal section are as follows: (a) *Slenderness ratio*, l/r; (b) *End conditions*—flat or pin, and pins in two directions, parallel and perpendicular to web; (c) *Quality of steel:* 60,000, 70,000, nickel.

2. With the ideal form of column constant, we should in turn

Mr. Worcester. make (a), (b), and (c) variables, assuming for each series the other two constants. For instance, for the (a) series, let us take for (b) flat ends, and for (c), 60,000-lb. steel. Then try (a) for the following values of l/r: 25, 50, 75, 100, 125, 150, 175 and 200. This involves $8\times 3=24$ tests.

Then taking (a) at, say, 75, and (c) 60,000-lb. steel. Try the three conditions of (b) involving $2\times 3=6$ tests in addition to three already made.

Then taking (a) as before, and (b) as flat ends, try the three grades of steel, involving $2\times 3=6$ additional tests.

Here we have 36 tests necessary to determine the essential points about which we are in doubt.

3. The points which cannot be determined by the ideal column may be classified under the heads of form and details. In testing to determine the effect of these elements, we should wholly eliminate the variables previously considered. That is, we should adhere to one grade of steel, one form of end and one value of l/r.

In certain forms the question of details is necessarily eliminated. I-beams, H-sections, single L-sections, and T-sections, of single rolled members, may be said to be in this class. It is not easy to see just how many tests should be made to cover these, but perhaps 3 I-beams, 2 H-sections, 4 Ls (of different widths and thicknesses of legs) and 2 T's would suffice, or $11\times 3=33$ tests.

In the built I-section, tests should be made for varying pitch of rivets, varying thickness of web, and varying thickness of flange in proportion to width. Allowing 4 values in each of these cases, we would require $(4+3+3)\times 3=30$ tests, though 3 of these might have already been made, leaving 27 new ones.

In the double-channel section there are many combinations of detail and form (taking the addition of a cover plate into consideration). How many are necessary to determine the effect of latticing and end plates, it would be presumptuous to prophesy in advance of the report of the sub-committee having this matter in hand, but it should be possible to make these with all other elements constant, and then select the details making the most perfect column with which to try out the variations in cover plates.

4. The above outline calls for 96 tests plus those required on channel sections. It is not to be disputed that more are desirable,

as we cannot acquire too much information along this line. If resources are available, by all means let us make all the tests we can. Nevertheless, it should be carefully considered whether it would not be wiser to make more than 3 tests of a kind according to such a program as the above, rather than to throw in extra varieties which partially duplicate results, but are not wholly applicable to the schedule because of certain other variables. **Mr. Worcester.**

Rejoinder on the Part of the Committee to the Above Discussion.

The Committee desires to express its appreciation to the participants in the discussion of the preliminary program of tests of steel columns for many valuable suggestions which will receive careful consideration in the development of the program, particularly in connection with what have been designated "special" as distinguished from "standard" tests. Limitations of time and space preclude the possibility of a detailed reply to the many questions raised in the discussion, and the Committee will therefore confine itself to the consideration of only a few of its principal features, especially criticisms unfavorable to the report.

It is particularly gratifying to the Committee that the discussion is, in the main, commendatory of the program as outlined, although two of the writers, Mr. Churchill and Mr. Lindenthal, anticipate little useful information from the tests. Mr. Churchill considers the program satisfactory in view of the small capacity of the testing machine, but inadequate by reason of that limitation. Mr. Lindenthal takes exception also to the program itself.

Several writers call attention to the urgent need of a testing machine of at least 10,000,000 lbs. capacity. Mr. Lindenthal suggests a capacity of 50,000,000 lbs., and a machine of the type proposed by the late Colonel Flad, which is also recommended by Mr. Derleth. As stated in the introduction to the report of the Committee, a resolution was adopted by unanimous vote at the first conference held on September 24, 1907, that the enlargement of the present testing facilities at the Watertown Arsenal by the erection of a machine of at least 10,000,000 lbs. capacity is desirable. However, in the judgment of the Committee, any large testing

machine that may be constructed should be accurate, and of a permanent rather than a temporary character; and it should be adapted to a considerable range of length.

It is to be borne in mind, however, that the cost of an extended series of tests on very large compression members is practically prohibitive. Numerous tests on smaller sections are therefore indispensable in an effort to establish a broad foundation experimentally for compression formulas applicable to the wide range of conditions encountered in practice. It is obviously of the highest importance that such tests should be supplemented by others on much larger sections as soon as increased facilities may render this feasible. With the data afforded by numerous tests on smaller sections as a groundwork, comparatively few tests on larger sections will suffice to furnish information within reasonable limits of certainty, applicable to the design of large compression members. In the judgment of the Committee, therefore, there would be ample justification for the present program of tests even though a much larger testing machine were available at the present time.

Several writers suggest tests on models of large compression members. This procedure is open to serious objections, however, aside from the cost of fabrication, owing to unavoidable differences in the character of the material and workmanship.

Mr. Lindenthal lays emphasis on the importance of comparative tests between different types of columns of like sectional areas and radii of gyration, on the ground that there is no experimental evidence to warrant the usual assumption that such members are equally strong. Referring to the tests planned by the Committee, he asks, "What useful knowledge will they furnish that we do not already have, little as that is?" Mr. Lindenthal appears to lose sight of the fact that the "assumption, universally accepted," governing the design of compression members, in its broadest interpretation, concerns itself only with unit stresses, irrespective of sectional areas. It is assumed that for like materials and end conditions, the unit stresses are constant for like slenderness ratios irrespective of sectional areas. The tests have been planned largely with a view of determining the validity of this assumption. For that reason slenderness ratios are prescribed, varying by regular increments, but identical for columns of

different types. An equally important consideration is, however, that the range in the values of the slenderness ratio should cover the limits observed in practice. While the upper limit is commonly fixed at 100 in the best bridge practice, limits of 150 and even higher are of frequent occurrence in building construction and in lateral bracing. It becomes, therefore, a matter of great practical interest and importance to determine by actual tests on sections in common use the extent to which the allowable unit stresses should be reduced for these higher ratios. In some instances, as, for example, in the case of towers for electric transmission lines, length-ratios well above 200 are not uncommon. For that reason, and as a matter of scientific interest in its bearing on the theory of column formulas, one set of tests has been planned for length-ratios up to 350, and for three different grades of steel. The sections proposed by Mr. Lindenthal are not only far in excess of the capacity of the machine, but would restrict the slenderness ratio to about 75 for the available distance of 26 feet between cross-heads. Moreover, the types suggested are in part of an unusual character, or faulty in their design. For example, the outstanding legs of the 8 x $3\frac{1}{2}$ x $\frac{7}{16}$-in. angles in Section 2 would fail by local buckling before failure in the column as a whole could be brought about, and that would probably be true also of the 18 x $\frac{3}{8}$-in. cover plates in Section 3. The specifications for steel structures of the American Railway Engineering and Maintenance of Way Association limit the ratio of the unsupported width of outstanding parts of compression members to thickness of metal to 10. Wholly apart, however, from these criticisms of the particular sections Mr. Lindenthal proposes, it is obviously impracticable to design a set of sections, widely differing in type, and meeting the requirements as to equality of area and radius of gyration, without considerable sacrifice in the possible range of slenderness ratios, within the length limit of the machine. For example, the built I section in Schedules Nos. 1 and 2, consisting of a 10 x $\frac{3}{8}$-in. web and 4 angles 4 x 3 x $\frac{3}{8}$ ins. has a minimum radius of gyration of only 1.65 ins. thus admitting of tests up to a slenderness ratio of 175. A corresponding radius of gyration would limit double-channel sections to a depth of 4 ins.

While it would doubtless be interesting to make a few tests under the conditions suggested by Mr. Lindenthal, the Committee

can see no justification for making this scheme a basic one in its inquiry. The purpose of this inquiry is not, as Mr. Lindenthal seems to infer, "to find another formula giving perhaps closer agreement with theory" than the straight line formula, but to determine for the first time in a comprehensive way the actual strength both elastic and ultimate of typical column sections, for 60,000 and 70,000 lbs. carbon steel and nickel steel. With the exception of four columns tested by Mr. C. P. Buchanan, six columns tested by Mr. J. A. L. Waddell, and two models of chord members of the Quebec Bridge tested at Phœnixville, no experimental data whatever are available on the strength of riveted compression members of open-hearth steel. The need of such data is, indeed, so pressing that it was the unanimous sense of the engineers present at the conference in New York on September 24, 1907, referred to in the report of the Committee, that in the proposed tests of structural materials, the investigation of the strength of steel compression members, in a comprehensive manner, was of paramount importance, and should receive first consideration.

In the light of the data that these tests are expected to yield, the values of the constants in the straight-line, or any other formula, may be determined independently, in his own way, by anyone interested in the subject. The results of the tests thus far made serve to indicate clearly that the constants in common use in connection with straight-line formulas lead to working unit stresses that are unjustifiably high.

Mr. Lindenthal mentions four examples among the "multitude of other tests that seem to be irrelevant and unnecessary." In reply it may be said:

1. That eccentricity of column loading, especially in building work, is frequently unavoidable. By the use of movable pin bearings, the bending moments induced by the eccentric application of the load is definitely determinable, and the validity of the formulas in use for combined compression and flexure may thus be verified. In connection with the "special tests," it will also be of interest to ascertain, by the use of such movable bearings, the agreement of the geometric center of gravity of the section with the gravity axis determined by successive loadings with varying eccentricities, and the effect of such small movements on the elastic behavior and strength of the member.

2. The tests for columns lightly spliced with imperfect end contact, as well as tests on the efficiency of intermediate batten plates, are proposed for the reason that there are innumerable instances of such details, and their number is constantly increasing, in spite of the fact that their employment is recognized as bad practice. A few tests to determine their influence on the strength of a compression member are desirable, both as a caution to designers of new work, and as an aid to engineers in passing judgment on the strength of existing structures.

3. A study of the effect of tight and loose riveting is concededly of minor importance, and it is doubtful that such tests will be actually undertaken.

The importance of using multiple compressometers to which Mr. Deans calls attention is recognized by the Committee and referred to in paragraph 18 of the Preliminary Program. The values of both the elastic limit and the yield point will be reported for each test.

Mr. Gifford calls attention to the recommendation of Mr. Bird that an investigation be made of the relative strength of similar cross-sections having like radii of gyration, but different thicknesses of metal. Mr. Merriman cites some experimental work in that connection, and points out that the program seems well arranged to throw light upon this important matter.

Both Mr. Frye and Mr. Bird suggest the desirability of tests on columns under partial constraint at the middle section. Since such conditions are of frequent occurrence both in bridge and building work, and few, if any, experiments in that connection have been made, the suggestion is a natural one. A little consideration will indicate, however, the extreme difficulty of bringing about conditions on a column in a testing machine truly comparable with the conditions to which such columns are subject in service, and there is the added difficulty of ascertaining the effect of such conditions on the recorded loads at the extremities. It is apparent also that the results will be very largely influenced by the effectiveness of the details at the center designed to preserve the alignment of the column. In practice such details vary from an effectiveness of little more than zero to a near approximation to fixed-endedness.

In reply to Mr. Lucius the Committee desires to state that

it is intended to determine the moduli of elasticity in tension and compression of all material used, by tests on small specimens, as stated in paragraph 12 of the Preliminary Program.

Mr. Derleth and Mr. Mills inquire why the prescribed position of the pins in many of the tests is parallel to the web. Although the clear width between webs in the double channel sections, both rolled and built, is given such a value as to make the radius of gyration about an axis parallel to the webs a little greater than about an axis normal to the webs, yet it is anticipated that failure for the flat and fixed end conditions will take place in the plane of the latticing. If that should prove to be the case, the pins should be placed parallel to the webs in order that the results may be comparable with those for other end conditions. It is desirable, however, that at least a few tests should be made for pins in a direction normal to the webs.

Mr. Quimby directs attention to an important matter, namely the effect of the time element on the results. It is hardly apparent, however, why this should influence the values within the elastic limit in the case of a material having a well-defined elastic limit, such as steel, although Mr. Quimby states that recent tests under compressive loads "continued for hours and for days, developed unexpectedly low values, both elastic and ultimate." It would seem very important to obtain at least such information on this important point as may be afforded by a few tests, the time-element involved precluding the possibility of an extended series of such tests.

The Committee fully concurs in the views expressed by Mr. Worcester that the multiplication of variables in comparative tests should be avoided. In arranging the program for standard tests this requirement has been kept carefully in mind. In one series, for example, the only variable is the slenderness ratio. In another series intended for comparative purposes the first series is duplicated, the only variable being the end conditions. It is then proposed to duplicate this double series by making the character of the material the only variable. Mr. Worcester's recommendation seems to be founded on the assumption that if a series of tests for varying slenderness ratios be made under uniform end conditions, and for material of a given grade, then the effect of variations of end conditions may be adequately determined by making

tests for only a single slenderness ratio. Again, that the effect of varying material may also be satisfactorily determined by tests for a single slenderness ratio.

This assumption is, however, not borne out by existing experimental data, which seem to point to the necessity of making complete series of independent tests for varying slenderness ratios in order to establish the effect of variations in end conditions and in the material. Christie's tests, for example, show that the relative behavior of struts of high and medium steel and wrought iron is very different for different slenderness ratios.

Mr. Talbot's apparent inference that the tests will be directed especially to the determination of the ultimate strength of the members is erroneous. Careful observations will be made on the elastic behavior of each specimen, and the results reported.

The discussion relates in large part to the testing of details of various kinds. The Committee desires to state that the first object of the tests is to obtain reliable data on the behavior and strength of typical compression members fabricated in conformity with the best current shop practice, and that in the proposed subsequent consideration of tests of details careful attention will be given by the Committee to the suggestions advanced by various writers, for which the Committee desires again to express its appreciation. A detailed discussion of these suggestions is regarded as premature at the present time. It may be said, however, that many of the suggestions regarding the testing of latticing are, in the judgment of the Committee, impracticable with a machine of the present limited capacity, and that exhaustive tests on latticing can be satisfactorily made only on a machine of much larger capacity.

SOME RESULTS OF THE TESTS OF STEEL COLUMNS, IN PROGRESS AT THE WATERTOWN ARSENAL.

By James E. Howard.

The results here submitted have been obtained on material representing a part of a comprehensive series of tests on steel columns in which the testing laboratory is working in coöperation with and aided by a Committee on Structural Shapes. The composition of this Committee is stated and the general program of tests given in another paper which is being presented concurrently herewith.*

It may be said, almost axiomatically, that the ultimate strength of iron and steel compression members, of usual engineering proportions, is limited by the elastic limit of the material.

In the case of short compression specimens, those which are only one or two diameters in length, the ultimate resistance is much above the elastic limit; in fact in such a specimen there is practically no limit of ultimate resistance, since the metal yields by direct compression and lateral flow incident thereto until the frictional resistance between the ends of the specimen and the platforms of the testing machine becomes so great relatively that distortion ceases and a state approaching cubic compression is reached beyond which no further displacement occurs.

There is an intermediate stage, however, in which continuous flow may occur under a given pressure, during which a constant ratio is maintained between the total compressive stress and the sectional area of the sample; that is, the stress on the steel in pounds per square inch remains the same during this period. This stress was found to be about 85,000 lbs. per sq. in. for a grade of medium soft steel.

With the higher stresses and certain of the phenomena which accompany them a series of column tests has little to do, although it is desirable that the behavior of the steel in these short lengths

* See p. 282.

be known as an aid to the better understanding of features which may influence the resistance of columns of full size.

Importance attaches to conditions of internal strains in the built members, both to those which are inherent to many sections and are due to conditions attending the manufacture of the shapes and to those which are due to the subsequent manipulation down to the time of assembling the parts of the finished columns. The surfaces of hot metal first to cool are left finally, as to their internal strains, in a state of compression, the intensity of which depends upon the rate at which the cooling is effected. It follows that the presence of internal strains of some degree, may be expected in compression members in general. Permanent sets will begin to develop when the applied stresses correspond to the difference between the elastic limit of the steel and the internal strains pre-existing in the metal. Such permanent sets will be of a local character but they seem adequate to account for some of the early sets of moderate extent, frequently witnessed in column tests. They have their influence, such as it is, in promoting early failure of compression members.

The shape of the compression stress-strain curve also has an influence on the ultimate resistance of a column. A jog in the curve at the elastic limit, the steel for a time yielding under reduced stresses, might lead to the prompt failure of the compression member, if the elastic limit was reached even locally.

Internal strains result from straightening members when cold. Some parts must perforce be strained beyond the elastic limit by the operation of gagging in order that the straightening process be effective. Internal strains resulting from this cause act in a limiting manner, the same as those caused by thermal changes only, the remaining part of the elastic limit being available for the resistance of applied compressive stresses without causing the development of permanent sets.

The material of the present tests was affected by the presence of internal strains and early sets occurred in consequence.

Internal strains may be present in built columns, due to manipulation at the time of assembling the several parts. Their influence on the compressive resistance and the virtual lowering of the elastic limit of the column as a whole would be the same as the presence of internal strains resulting from the other causes enumerated.

When the several above causes tending to weaken the compressive resistance of the metal are eliminated wholly or in part there is reason to expect that different values will be experimentally found in the tests of structural members, and that higher values will pertain to the grades of steel at present used.

Referring now to the tests in progress and the results obtained, tests have been made upon annular sections of lap welded steel tubing, 5 ins. diameter, with walls nominally 0.36 ins. thick, also tests with some 6-in. wide-flange H sections. The tests have been

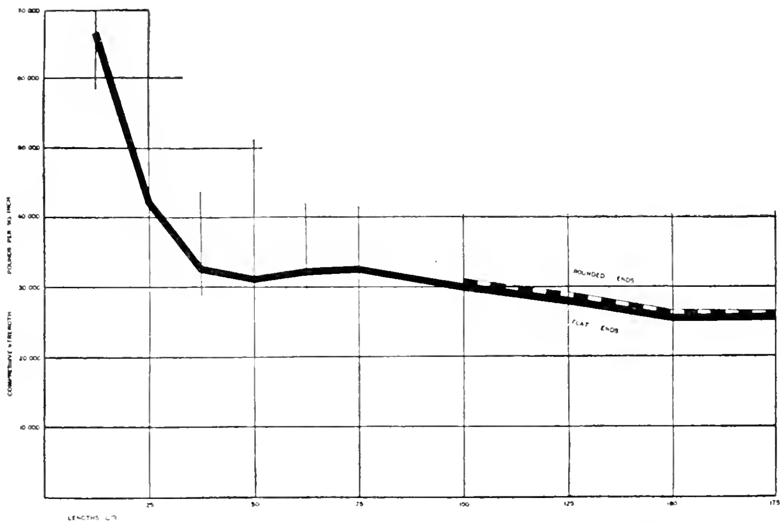

FIG. 1.—Compressive strength of annular sections, lap-welded steel tubing, for values of l/r ranging from 12.5 to 175. Full lines represent the strength of columns with flat ends; dotted lines, columns with rounded ends.

made with flat ends in each of these shapes and with the annular columns tests have been made also with rounded ends.

Diagrams have been prepared on which are plotted the principal features of the tests. In Fig. 1 are shown the ultimate strengths of the annular columns, the curve representing the mean of three or more tests of each lengths.

The elastic limit of the metal of the tubing ranged from 29,000 to 33,000 lbs. per sq. in., jogs in the stress-strain curve occurring

under loads two to three thousand pounds per square inch beyond the elastic limit.

An inspection of the diagram shows the curve representing the ultimate strengths, for values of l/r from 37.5 to 175, to be approximately a straight line, with a downward slope. Just above the full line of the diagram appears a dotted line, the latter representing the strengths of columns having rounded ends. In these tests the strengths displayed by columns with rounded ends exceeded those with flat ends, the difference not being great, no more than might be expected among others of this lot of material, with the same kind of end bearing. The spherical ends of the fixtures used

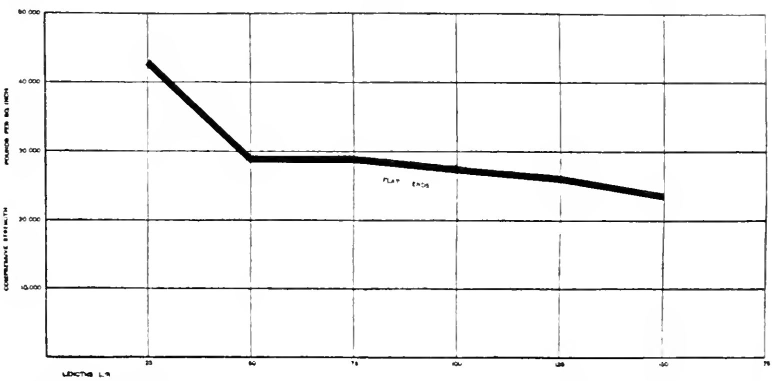

FIG. 2.—Compressive strength of wide-flanged, rolled steel H sections, for values of *l* *r* from 25 to 150.

in these tests had a radius of curvature of 2.75 ins., resting in seats having a radius of curvature of 3 ins.

The results with the wide-flanged, rolled H sections with 6 x 6 x $\frac{1}{2}$-in. flanges and $\frac{1}{4}$-in. web, are shown in Fig. 2. The shape of the curve corresponds well with that representing the annular columns.

For comparison, Fig. 3, was prepared, illustrating some earlier results obtained with Phœnix columns of wrought iron. These were 4-segment columns, 8 ins. diameter with a sectional area of about 12 sq. ins.

It will be noted that the curve of these tests is not materially unlike those obtained in the present tests of steel columns. The

Phœnix columns, however, developed a somewhat higher strength along that part of the curve representing the longer columns of the series.

In order to illustrate other features of the current tests Figs. 4 and 5 were prepared. Fig. 4 shows the curve for the flat ended columns of Fig. 1, and two additional lines referring to the behavior of these columns after the maximum loads were passed, and at a time when the compressive resistance was in a diminished state.

Observations were made and the compressive resistance noted at the time the ends of the columns ceased to remain in contact

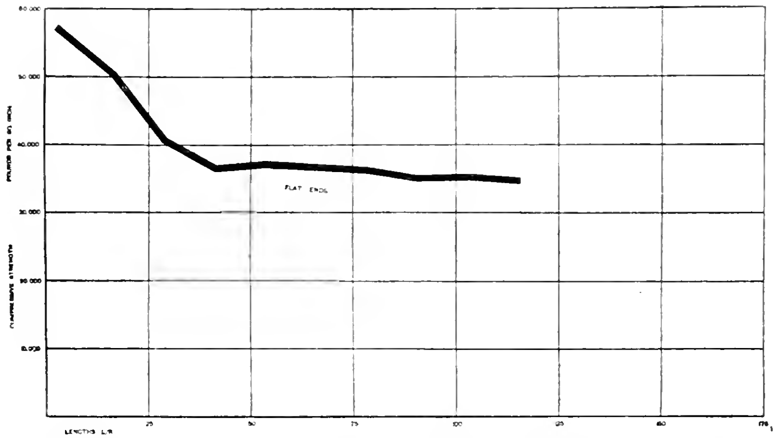

FIG. 3.—Compressive strength of wrought iron Phœnix columns. Four-segment columns, 8 ins. diameter. Tests of 1879.

with the platforms of the testing machine over their full surface and at the same time the lateral deflections at the middle of the lengths of the columns were noted as they occurred. Those observations had to do with the question of the relative behavior of columns with flat and with fixed ends. It would seem that a flat ended column is virtually a fixed ended one so long as the ends remain firmly in contact with the platforms of the testing machine and continue to sustain a compressive load on all sides.

The several columns remained in contact with the platforms of the testing machine after the maximum stress had been passed and until there was a decided loss in resistance, accompanied by

comparatively large lateral deflections. From this behavior it is inferred that no higher strength would have been displayed by these columns had their ends been fixed instead of flat.

In Fig. 5 is reproduced that part of Fig. 1 which refers to the strength of the columns with round ends, with two additional lines illustrating their behavior subsequent to the determination of the ultimate resistance. It has been observed in earlier tests of columns with pin ends that certain of the longer ones deflected in a sudden manner soon after the maximum stress was passed, after which sudden springing only a small part of the full strength

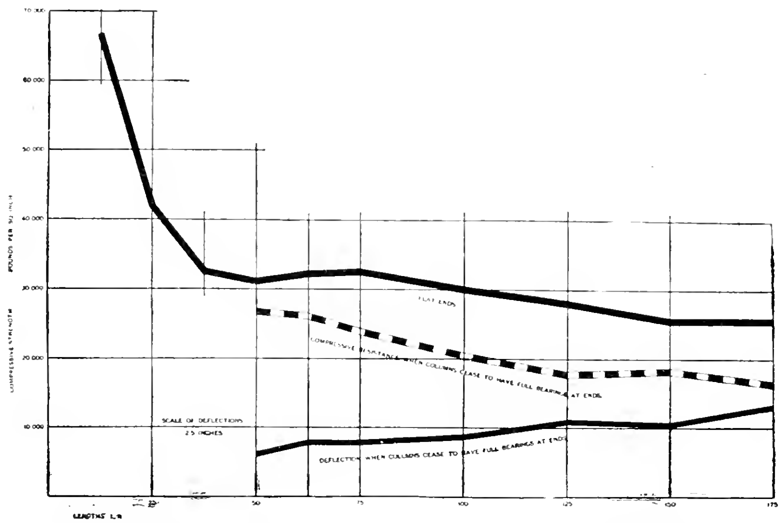

FIG. 4.—Curve of compressive strength of annular sections, lap-welded steel tubing, and curves showing the diminished resistance of the columns and the amount of deflection when they ceased to retain full bearings at their ends against the platforms of the testing machine.

remained. This feature deserves consideration when judging of the relative safety of different columns against the effect of side thrusts from accidental causes.

In the tests with rounded ends similar behavior was noted. After passing the maximum stresses the columns deflected gradually for a time, until the resistances shown by the dotted line of the diagram were reached, whereupon there occurred a sudden spring-

ing, after which the resistance was found as shown by the lower full line of the diagram. In the case of the columns of the longest lengths tested the strength of the bent column after springing was less than one-third of the primitive compressive resistance.

Respecting the general indications of these tests, it appears that the shorter columns, those in which l/r is less than, say 37.5, develop strength in excess of the elastic limit of the material, whereas those of greater lengths do not exceed and may not reach the elastic limit of the material considering the metal as a whole. Whether the ultimate resistance falls much below the elastic limit depends largely, it would seem, upon the initial condition of the column as regards straightness and freedom from internal strains.

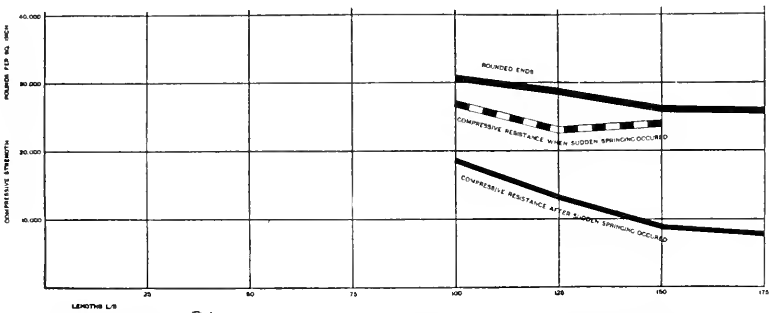

FIG. 5.—Curve of compressive strength of annular sections, lap-welded steel tubing with rounded ends, and curve of diminished resistance, after passing the maximum loads, when deflections suddenly increased; also curve of strength remaining after sudden springing occurred.

None of the columns in these tests were exempt from these influences, and the downward direction taken by the several curves is largely attributed to this fact.

The tubular columns were not straight prior to testing; long bends and short ones were present locally and the relative efficiency of the tubing as a strut could be judged of before the commencement of the test.

A short tubular column may be expected to take the shape shown by Fig. 6 when the loads are carried beyond the elastic limit, bulging at each end. The frictional resistance of the platforms of the testing machine restricts lateral expansion of the tub-

ing at the ends. In consequence of this action the stresses come upon the walls of the specimen in a slightly oblique direction, resulting in the formation of a ring of bulged metal near each end of the tubing. The influence of this action and its application to other types of compression members will be obvious to all.

FIG. 6.—Short annular column, showing ends bulged during testing. Typical shape assumed by short cylindrical specimens.

Fig. 7 shows a longer tubular specimen after the completion of a test. Commonly the maximum deflection occurs at the middle of the length of the column, although local bends may cause the place of greatest deflection to be somewhat eccentric. At the time of reaching the maximum load the distortion of the column is much less than shown by the cut.

FIG. 7.—Annular column. Typical shape after failure by triple flexure. Stresses continued after passing the maximum, increasing the distortion of the column to the amount here shown.

The effect of cold straightening is shown in Fig. 8, where lines of disturbed scale on the surface of the web of a wide-flanged H section are indicated. Columns of this type developed early sets during the tests. The internal strains introduced by the straightening process restrict the range of loads which may be applied before reaching the elastic limit of the steel and consequently tend to lower the ultimate resistance of the rolled shape as a strut.

Certain proximate causes are recognized which tend to lower the ultimate resistance of compression members, and it further appears that some of them admit of being corrected or avoided with varying degrees of success. The shape of the curve representing

FIG. 8.—Steel, wide-flanged H section, showing lines of scale on surface of web disturbed during cold straightening.

the ultimate resistance of columns of different lengths depends, it would seem, upon these factors. The results of these tests apparently show that centrally applied or axial loads may be sustained with equal success without regard to the type of end bearing.

SOME PRACTICAL APPLICATIONS OF METALLOGRAPHY.

By William Campbell.

There are many people who still look upon metallography as a fad and often with good reason. To them it has no practical application to-day; they candidly tell you that they made good steel long before metallography was thought of, or that they have looked into the matter and it proved to be of no value.

It was not so long ago that the same thing was said of the chemist, and as regards cast-iron we still meet practical men who have no use for chemistry around the plant. However, most of us realize the importance of the chemist and his work even in the foundry. Many of our best engineers also recognize that metallography has its practical applications as well as its fascinating theoretical interest.

One of the chief applications of metallography is the recognition of faulty structure and hence the cause of failure. It is eminently a method of testing, and its purpose is to coördinate with physical and chemical tests and not to replace them.

Some have objected to metallography because of the necessary cost, etc. In truth the first cost is very small compared with other methods of testing and the time used is small also. It requires no long and arduous course of preliminary training, but it does require constant thought and common sense.

The methods of preparation of the specimen are well known. The main object is to obtain a perfectly smooth surface free from scratches, in a minimum amount of time.

For the development of structure very many methods and reagents have been advocated from time to time. After polishing alone, slag in wrought iron, manganese sulphide and silicate in steel, graphite in cast-iron, etc., show up dark. Copper oxide and sulphide in copper, and various metallic compounds in alloys are colored. For etching iron and steel, a 5 per cent. solution of picric acid in alcohol is used. This turns the pearlite brown or black but does not color the other constituents. Near the eutectoid point it

is often hard to distinguish between the thin envelopes of ferrite and cementite in steels. A hot solution of picrate of soda turns the cementite black. To show up the grain of the ferrite in wrought iron a 10 per cent. solution of nitric acid in water gives good results. For quenched or hardened steel 4 per cent. nitric acid in amyl alcohol is to be recommended. In cast-iron, Fe_3C is distinguished from Fe_3P by heat tinting, though it is probable that as good results can be obtained by using the picrate of soda solution. Lastly, for alloys such as bearing metals, white metals, etc., 10 per cent. nitric acid should be used; for those rich in copper the acid should be just strong enough to etch the surface.

While the final structure of our material is very important, we are often liable to overlook the importance of the influence of all those changes which take place between the solidification of the metal and its final state. The more fusible metals serve as illustrations of how metals freeze. The dendritic structures seen on the surfaces of ingots of aluminium, antimony, etc., and the granular structure which appears on etching lead, tin, and zinc ingots, are reproduced in steel. We get a fine-grained structure with rapid freezing, whereas with slower cooling through the freezing range and a freedom to grow we find the dendritic or pine-tree crystals more pronounced as shown in pipes and cavities. As in alloys we have the location of impurities among the dendrites and between the grains, as, for example, the segregation of carbon, phosphorus, manganese sulphide, etc.

The effect of rolling and annealing is to destroy the pine-tree crystal or dendrite. Diffusion of the dissolved impurities such as carbon, phosphorus, silicon and manganese results in more or less uniform grains which on cooling down to the critical points split up into the different constituents, ferrite or cementite, and pearlite, according to their composition.

Such material as manganese sulphide, slag, etc., being mechanically enclosed, is merely rolled out into plates or filaments. There is a prevalent idea that iron sulphide is undesirable because it crystallizes out after the steel and forms a brittle envelope to the grain, whereas manganese sulphide crystallizes out before the steel and is therefore enclosed in it and has no such harmful effect. This is quite erroneous because manganese sulphide is fluid long after the steel has frozen. The real reason is that iron

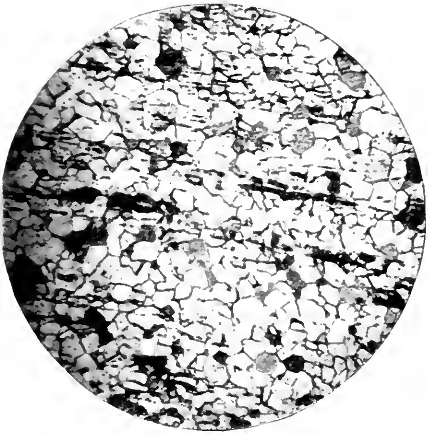

FIG. 1.—Wrought Iron, deeply Etched, showing Ferrite and Slag. (×32.)

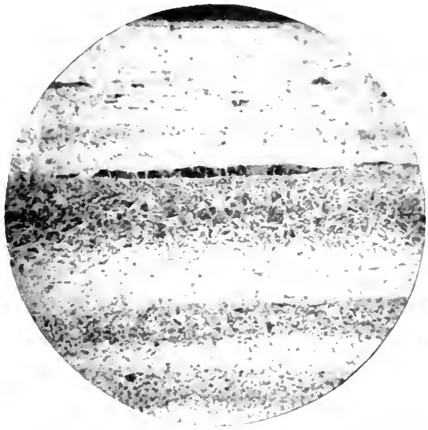

FIG. 2.—Charcoal Iron, showing Ferrite, Slag and Pearlite Bands. (×36.)

FIG. 3.—Wrought Iron Tube, showing Ferrite and Slag. (×58.)

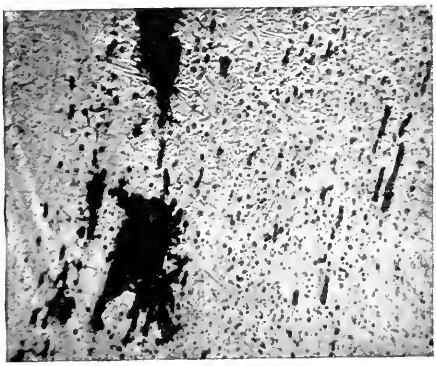

FIG. 4.—Wrought Iron Tube, like that in Fig. 2, supposed to be Steel. (×53.)

sulphide is soluble in molten iron, while manganese sulphide is not, and may be likened to an emulsion for it occurs in globules. These are drawn out during rolling while they are plastic. When they become hard the rolling in general makes the steel flow around them unless they are very large in size. Even in cold-rolled material they are often unbroken.

In wrought iron the behavior of the slag is somewhat similar. While plastic it is rolled out into plates or threads, which may or may not be broken up, either while plastic or when solid, depending on their size. The microscope has cleared up our ideas on the structure of wrought iron. The metal itself is not fibrous but is

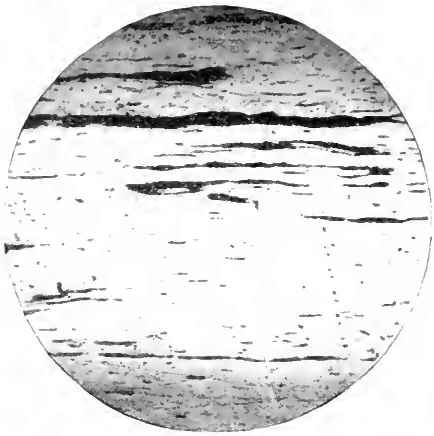

FIG. 5.—Wrought Iron, Unetched, supposed to be Open-Hearth Angle Iron. (×40.)

built up of grains of ferrite or pure iron. Throughout the mass we have more or less entangled slag which causes the fibrous appearance of the fracture. Fig. 1 shows a piece of wrought iron used for pipe-making. It has been deeply etched with 10 per cent. nitric acid to show up the grains of the ferrite, some of which appear dark on account of their orientation. The long black films are slag.

The microstructure often gives us a clue to the process of manufacture. For example, Fig. 2 shows a cross section of a piece of pipe-skelp made from charcoal iron. It has been etched with picric acid solution and shows in addition to a large band of

slag, several bands rich in carbon in the form of pearlite. The carburization took place in the hearth and by diffusion we have a perfect gradation into carbonless ferrite. Now in material manufactured by reheating scrap steel, etc., there is seldom such gradation and the areas of steel are more or less separated from the areas of iron by slag threads.

Fig. 3 shows a section of boiler tube, supposed to be steel, which failed. The cause is seen to have been the excessive amount of slag, the material being a poor quality of wrought iron. Fig. 4 is another section from a similar tube. Again, in Fig. 5 is shown a piece of angle-iron of so-called open-hearth steel which proved to be wrought iron made from a mixture of iron and scrap steel.

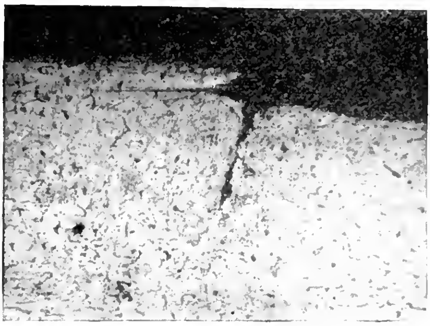

FIG. 6.—Butt Weld in Steel Pipe. (×58.)

As before, the large amount of slag was undoubtedly the cause of failure.

The difference between butt-welded and lap-welded pipe is also clearly seen under the microscope when the line of weld is traced. Fig. 6 shows a butt weld in a steel pipe. The view is near the surface and the film of slag is seen to be perpendicular to the surface of the pipe, whereas, had it been a lap weld it would be seen running diagonally across.

In mild and structural steel the microscope shows up the defects due to segregation and the like very markedly. The dark streaks which often tear on machining are found to contain thin particles of manganese sulphide. They are generally poorer in carbon than the rest of the mass and richer in phosphorus.

FIG. 7.—Cast Steel Crank, Unannealed, showing Ferrite and Pearlite. (×43.)

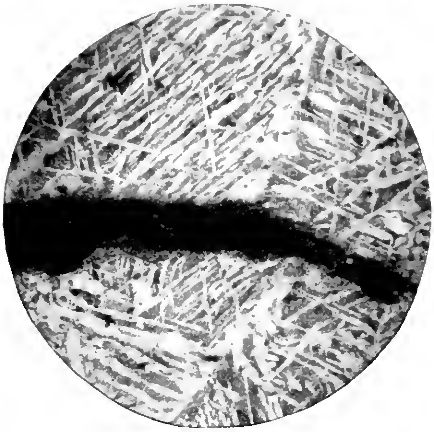

FIG. 8.—Cast Steel Crank, Unannealed, showing Flaw. (×43.)

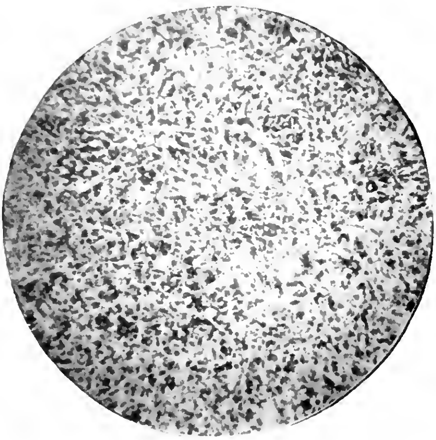

FIG. 9.—Cast Steel Crank, Annealed. (×43.)

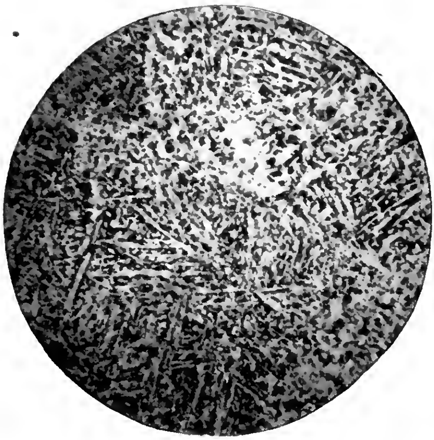

FIG. 10.—Cast Steel Crank, Annealed, showing a Trace of the Old Structure. (×43.)

The annealing of castings is of very great importance and here again the micro-structure is a very reliable guide. Steel as cast has a characteristic structure. Fig. 7 shows a section of a crank which failed. The structure is very coarse and within the grain the ferrite has separated out in a definite rectilinear way, resembling the Wiedmanstatten figures in meteorites. Fig. 8 shows a large flaw in the same material. If this steel was annealed, as was claimed, the temperature never reached the lower critical point Ac_1, say 700° C. A piece of the material was cut off and heated to just above the upper critical point, to 820° C., and slowly cooled. The resulting structure is shown in Fig. 9. In parts of the specimen, however, traces of the old crystallization were found, as shown in Fig. 10, showing that time had not been allowed for the changes to become complete.

Another example of faulty heat treatment is shown in Figs. 11 and 12, taken from a broken 8-in. crank shaft. Fig. 11 shows the structure near the surface while Fig. 12 is from near the center. They are both very coarse, due to overheating, and the carbon content of the center is evidently twice that of the outer portions, judging by the amount of the pearlite. In the very center a large mass of slag shown in Fig. 13 (unetched) was found. Fig. 14 shows a view near the surface of a similar shaft which failed. The structure is coarse and the interior showed a similar enrichment in carbon, though the analysis was given as 0.27 per cent. carbon. In all of the above the ideal structure would be similar to that shown in Fig. 9.

It is well known that the time necessary for annealing will vary with the size of the casting. In some cases, even after long annealing at the correct annealing temperature, there still remain some coarse blotches of ferrite, generally connected with films or threads of manganese sulphide. The only way to get rid of such a structure is to heat to 1000° C. ± which breaks down the coarse grain due to casting and at the same time overheats the steel. This overheated structure can then be refined by reheating to just above the critical point $Ac_{2\cdot 3}$ say 750 to 800° C.

Although the rate of cooling has but little effect on the size of grain, it does affect the grain structure and hence the physical properties. Slowly cooled material shows the normal amount of ferrite and pearlite corresponding to 0.9 per cent. carbon. If

FIG. 11.—Outside of 8-in Crank Shaft. (×40.)

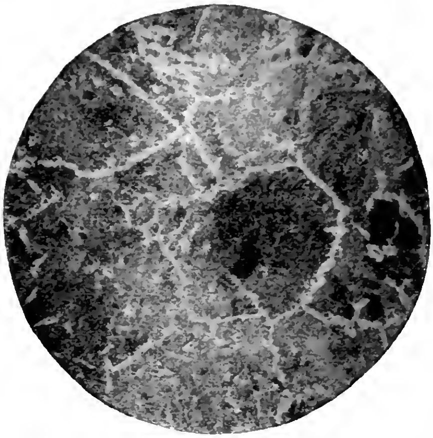

FIG. 12.—Interior of 8-in. Crank Shaft, high-carbon. (×40.)

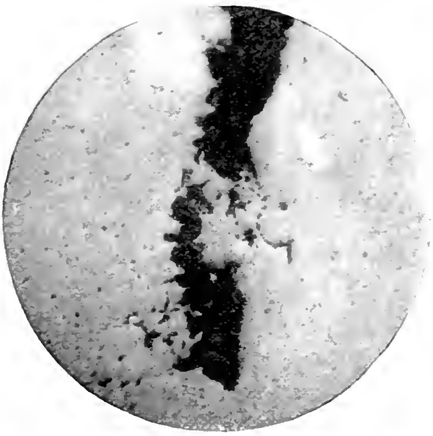

FIG. 13.—Eight-inch Crank Shaft, Unetched, showing Slag. (×40.)

FIG. 14.—Outside of Crank Shaft, 0.27 per cent. C. (×63.)

the cooling has been hastened (as by cooling in the air) as in the case of most rolled material, the amount of pearlite present fails to correspond with the theoretical 0.9 per cent. carbon composition. There is much more present: in other words its carbon content is low or its ferrite has not completely separated out (due to the presence of manganese). For such material the pearlite may run as low as 0.55 per cent. carbon, i. e. steel containing 0.55 per cent. carbon would show nothing but pearlite. The texture of the pearlite is a good indication of the rate of cooling through the critical range.

The effect of finishing temperature on grain size is not yet agreed upon. Many still hold to the idea that the higher the finishing temperature above the lower critical point Ar_1 (say 700° to 660° C.) the coarser the grain, which is the same as saying that to get a fine grain, finishing should be done near the critical point. On the other hand many thoughtful rolling-mill men will tell you that the properties of the steel depend on the amount of reduction in the rolls.

To try to arrive at some conclusion in this matter a number of temperature readings were taken with both the Fery and Wanner pyrometers and checked against a thermo-couple at one or two large plants. For steel rails the finishing temperatures as indicated by the optical pyrometers averaged 1050 to 1100° C.; for structural steel, 950 to 1000° C. And yet such material is not coarse-grained. (Reheating to such a temperature would give rail steel a very coarse grain.) A difference of over 100° C. in finishing temperature could not be detected in the size of grain but a difference in section could very soon be noticed. Similar results can be reached experimentally by rolling out small sections at different temperatures. A section heated to 1300° C. and rolled out with 30 per cent. reduction showed about the same sized grain as one heated to 1300° C., cooled to 900° and rolled out, the finishing temperature being about 700° C. Now, although the grain size shows small variations, the physical properties vary. The tensile strength shows a slight increase when finished near the upper critical point, but the ductility may in some cases be doubled.

Some idea of grain size can be gathered from Fig. 15, from head of a rail of small section, carbon about 0.45 per cent. The structure of a heavy rail, say 90 lbs., would be similar to Fig. 17.

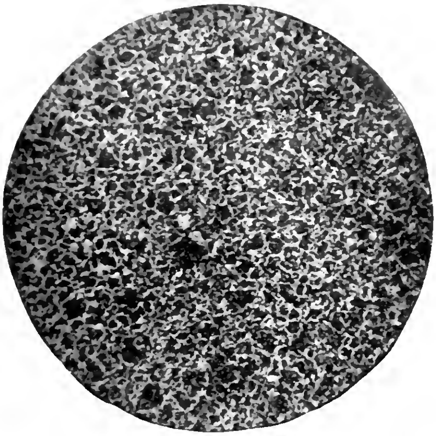

FIG. 15.—Small Rail-Head, showing Fine Grain, Ferrite and Pearlite. (×43.)

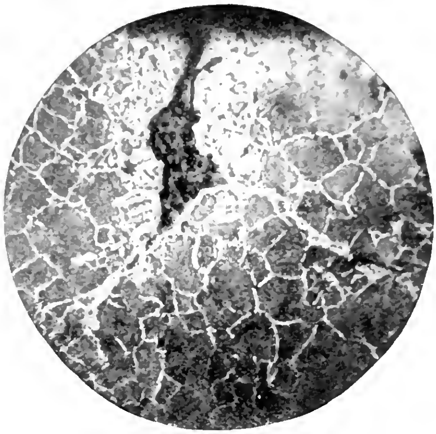

FIG. 16.—Flaw in Rail-Head, showing Ferrite, Pearlite and Slag. (×43.)

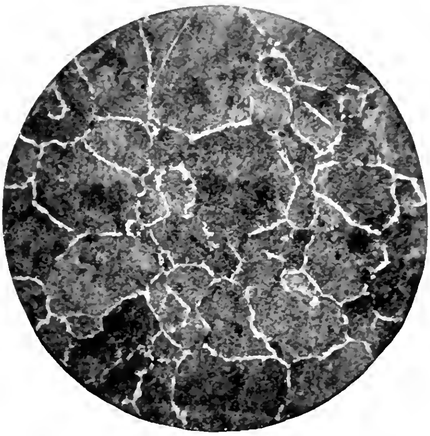

FIG. 17.—Normal Structure of Tire. Ferrite Envelopes around Pearlite Grains. (×70.)

FIG. 18.—Tire, coarser grain. (×88.)

Although only a comparatively few rails have been examined to find cause of failure the size of grain bore no relation to the number of breaks, in cases that came under our notice. Three marked causes of failure were found however. The first is shown in Fig. 16, the cross section of the head of a large rail. A mass of black slag has been rolled into the metal, which in its neighborhood shows strong decarburization. Such a structure would evidently cause failure. Similar cases of slag inclusions are common in the flange. The second apparent cause of failure was the presence of an abnormal amount of manganese sulphide, in some cases segregated into very coarse bands and accompanying banded material due to the segregation of carbon, phosphorus, etc. The last cause was deep seated and evidently came from piping. In the head and web of the rails marked segregation occurred; in the bands rich in ferrite much manganese sulphide (and silicate) was found, in some cases enough to give rise to a distinctly laminated fracture. One would naturally assume that the first failure could hardly be avoided, the ingot being imperfect at its surface to start with. In the second case, the time between adding the recarburizer and casting from the ladle had been insufficient to allow the manganese sulphide globules to float to the surface and enter the slag. The last case was due undoubtedly to insufficient discard during rolling.

Another interesting study in metallography is the structure and failure of steel tires. A number of tires, both good and bad, were examined. The size of grain varied, the average being represented by Fig. 17. Fig. 18 shows a coarser variety, while the structure of one of German make was similar to that of Fig. 15. The analyses and physical properties of all were remarkably alike. The faulty tires were examined at the point where the shelling out was worst. The presence of irregular bands of "slag," etc., was noticed in most cases. Fig. 19 shows a section unetched, the presence of such an amount of brittle "slag" being the undoubted cause of failure. Most of this occurred near the surface, and the pounding of the wheel on the rail would soon cause pieces of metal to break off. Figs. 20 and 21 show such sections after etching, the carbon is seen to be low in each. Fig. 22 shows the "slag" in place and at some considerable depth, while the structure of the steel itself is not normal. Certain of the tires showed the effect of

improper heat treatment as seen in Fig. 23 where we have the typical appearance of badly overheated or unannealed steel. In some cases the fault seemed to lie in the presence of much manganese sulphide in thin plates set in the usual areas rich in ferrite due to segregation. This structure is typically shown in Fig. 24, which might also be used to illustrate the same thing in rails. The normal structure of ferrite and pearlite is seen round the edge of the photograph, while the horizontal lighter band in the center encloses the manganese sulphide. In some cases of failure no inclusions were found but in the center of bands rich in ferrite a

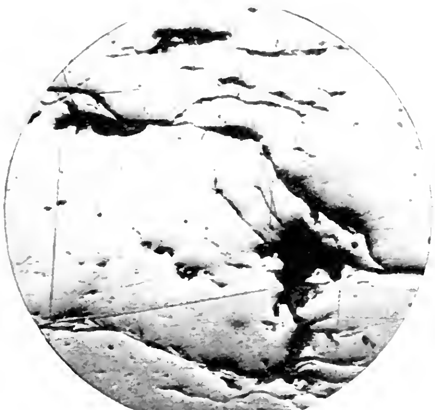

FIG. 19.—Faulty Tire, Unetched, showing Slag, near the Surface. (×70.)

thin parting was noticed, evidently caused by the rolling out of a blow-hole or gas inclusion. This initial weakness would finally cause shelling out of the tire.

In the examination of high-carbon steels the microscope is found to yield valuable information as to heat and mechanical treatment, and will sometimes show up the cause of failure when other tests have failed.

In its application to foundry work, metallography has not given us the results expected of it, but when as much work has been done on cast-iron as has been accomplished in the microscopic examination of steel, there is no doubt that the results will be as fruitful.

FIG. 20.—Faulty Tire, low in Carbon, showing Flaws. (×65.)

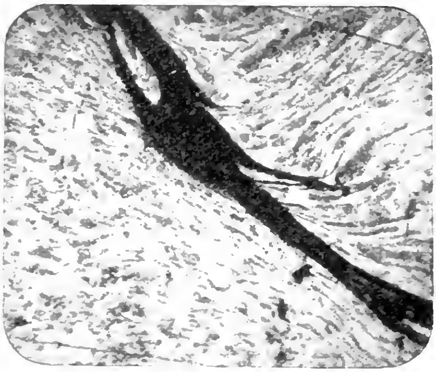

FIG. 21.—Faulty Tire, low in Carbon, showing Flaws. (×65.)

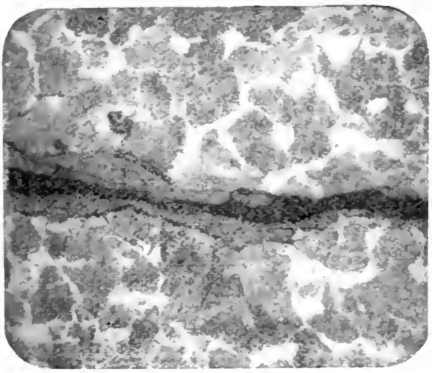

FIG. 22.—Slag Flaw in Tire. (×65.)

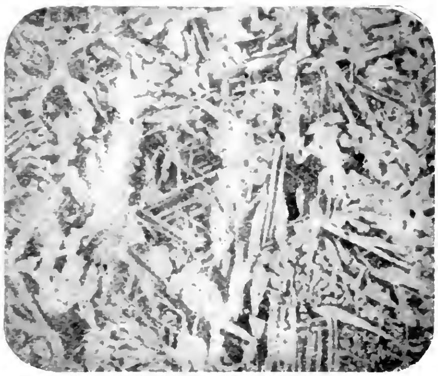

FIG. 23.—Tire with Coarse Structure. (×65.)

In the field of non-ferrous alloys, such as the brasses, bronzes and the like, metallography is being used with great success, for, as in iron and steel, good structure is as necessary as good composition to produce the required physical properties.

In conclusion, an apology must be offered for the lack of

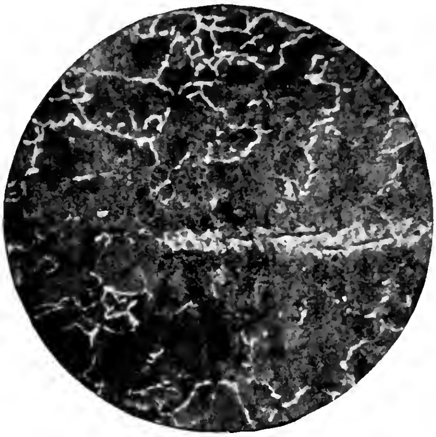

FIG. 24.—Tire, showing Ferrite, Pearlite and Segregation Band containing MnS. (×70.)

connection of this paper, which, however, is intended merely to show one or two cases where the microscope has been successfully employed in locating the causes of failure, a subject of no small importance to the engineer.

DISCUSSION.

Mr. Henry Fay.—I am interested in what Mr. Campbell has said in regard to grain size. Recently I have been examining a series of broken steel rails. There has been a certain school of metallographists who led us to believe that everything depended upon grain size. In a series of probably forty or fifty broken rails there is not one that I can blame on coarse grain. It was invariably some other cause. Incidentally I took occasion to look up the literature on the subject, and I found that in a series of papers on the heat treatment of steels there is this important fact: The tensile strength reaches a maximum invariably at a temperature somewhere near the lower critical point. A second maximum is also invariably found at about 1000° C., and steel which has been heated to this temperature is much coarser grained than that heated to the lower critical temperature. We know that under favorable conditions the grain size will increase with the rise in temperature. We have here a rather curious phenomenon—a maximum tensile strength and at a temperature where we ordinarily get coarse grain. The ductility is invariably greater at the higher temperature also. I believe the condemnation of material with coarse grain has been completely over-done. The relationship between grain size and physical properties deserves a great deal more study. **Mr. Fay.**

Mr. A. A. Stevenson.—I would like to ask Mr. Campbell in regard to his investigation of the tires in question, whether he knows the service in which the tires were employed, engine, tender or coach, passenger, freight or switching; whether he knows the wheel loads, the construction of the trucks, the character of the roadbed and the grades; whether heavy or light. **Mr. Stevenson.**

Mr. Wm. Campbell.—No, sir. I was simply given the section of the tire and asked to find out what was the matter with it. In each case the tire showed this large amount of slag or oxide where the shelling out occurred. **Mr. Campbell.**

Mr. Stevenson.—I believe any steel maker would be glad to find out how to completely eliminate slag and oxides from steel. **Mr. Stevenson.**

Mr. Stevenson. If there is any steel maker making open hearth steel free from slag and oxides, I would like very much to have some lessons from him.

As a possible explanation of the trouble we are having to-day from shelliness of tires, I would say that the theory that it is due to entrapped particles of slag in the steel was put forward in 1902 by our Engineer of Tests, Mr. Norris. This theory was followed up for a number of years but was finally abandoned, as careful examinations showed it to be true in only a few cases, and investigation and inspection of the wheels in service soon showed other and more prevalent causes. While we feel quite certain that we are on the right track, we are still open to conviction, and by no means desire to take the attitude that we are in no way responsible, or that we can do nothing more towards improvement. There are many puzzling features in the problem. We have found in tires made from the same long ingot on the same axle, that one develops shelliness and the other does not. We would hardly expect any marked variation in the amount of slag or oxides in tires from blooms adjacent in the same ingot. We have also had instances where such tires, after turning and putting back into service, have developed shelliness just the reverse of the first instance, which would seem to throw doubt on the slag and oxide theory.

We have also found from our field investigations that on the same railroad the same class of equipment pulling the same trains develops most of the trouble from shelly tires on one or two divisions of the road while the other divisions have very little trouble. We found that on these roads it was the low-grade divisions that had most of the trouble, while the mountainous divisions were practically free from it. In many instances, tires from the same heat were scattered all over the divisions. The records of these tires also showed that most of the shelliness developed on wheels under heavy tenders in through passenger service, and that only a small amount developed in freight service.

In the face of facts like the above, and after careful examinations of a great many shelly tires, by etching, by the microscope and in the field, studying the development of shelliness, we feel that we can say with a great deal of certainty that the slag and oxide theory can only account for a very small percentage of shelliness. The increased trouble from shelliness on some roads bears a direct relation to the enormous increase in wheel load.

We have come to the conclusion that this is not a problem that the steel or tire maker can solve alone but that it can only be solved by coöperation on the part of the railroads. We know that the cause cannot be determined off-hand by the examination of half a dozen rusty old shelly tires of unknown origin and condition of service. **Mr. Stevenson.**

THE PRESIDENT.—Before the days of microscopic examination of metals, after studying chilled cast iron car wheels at Altoona, I broached the theory that the reason for shelling out was slag in the metal. The foreman of our wheel foundry, who was a graduate of Lehigh University and a very bright man, took issue with me. We were both of us younger, and I was a good deal more tenacious even than I am now, and that is saying a good deal. And so we kept at it back and forth, each bringing out points to substantiate his view, until one day he put a man at work with a riveting hammer. A cast iron wheel was set up on edge and hammered in one place, a few thousand blows by this man and then by another and so on until after a while he got a shelled out spot under the hammer, and the metal was perfectly clean, so far as anything we could find by means of our pocket lenses, which was all we had at that time. I can't help saying that I think Mr. Stevenson has hit the nail on the head; that shelling out is not a simple problem but a complicated one. There are cases, and we can apparently find them in a cast iron wheel, where intermingled slag does play a part. I don't think there is much question about that. On the other hand, the service and the blows that metal receives are unquestionably an element. **The President.**

Let me add one other point. We have had a good many split chills. A crack starts circumferentially, not transversely, in the tread, and that crack will run around. I have seen wheels in which this crack extended half way around, and by and by the flange breaks off. We had one very interesting case of a frieight train that came into Trenton with about twenty-one inches of the flange gone on one of the wheels. Of course the whole division was set to work looking for that flange, and it was found twenty-seven miles back. The wheel had actually run twenty-seven miles with about twenty-one inches of the flange gone. Returning to the cracks, how do they start, and what is their cause? Is there anything that we can get at that will help us out in this matter?

The President. We develop a good deal at Altoona in the clash of mind with mind; one takes one view and another another; and we each stick to it until we can't hold on any longer. It is, we think, one of the best ways to develop truth. I maintained the idea that the crack was started by the heat produced when the brake shoe was applied. Strangely enough these cracks start towards an eighth of an inch below the surface. I have actually seen an incipient crack, when a wheel had been broken up for purposes of study, with nothing visible on the surface, and between the surface and the crack there was perfectly clean, fresh metal, caused by the break in breaking up the wheel. My idea was, that when the brake shoe rubs on the wheel the layer of the wheel next to the brake shoe, perhaps an eighth of an inch or less thick, is heated and the temperature is quite high locally. That would cause an expansion, for it takes some time, as Mr. West told us yesterday, for heat to travel even through cast iron or wrought iron. So the expansion of that thin layer of perhaps an eighth of an inch of metal on the surface would cause a rending action which would ultimately start a crack in the brittle white iron underneath. That was my theory. As I said before, there were other theories. Mr. Gibbs, the General Superintendent of Motive Power, enunciated the theory that it was caused by planing. The action of the wheel rolling on the rail, especially where it strikes a joint every time, is really a hammer blow, and this expands the surface layer and starts the hidden crack beneath. The fact that these incipient, non-visible cracks underneath the surface are blue when they are first discovered I regarded as a very strong confirmation of my theory that the crack was due to heat. But finally Mr. Gibbs sent down in great glee to me one day a crack produced artificially by planing with a hammer, which had all the characteristics of the natural cracks except the bluing, and claimed that the bluing might naturally be caused in service by a subsequent application of the brakes.

Mr. Stevenson. MR. STEVENSON.—One point I would like to emphasize is the fact that during the past two years our time has been taken up largely with the discussion of steel rails; and it seems to us that it is just as important that some time be devoted to the subject of steel tires. They work together, and the troubles of one are to a certain extent dependent upon the troubles of the other.

At the meeting last night our President brought out very clearly that as far as rail failures are concerned, the railroads feel Mr. Stevenson.

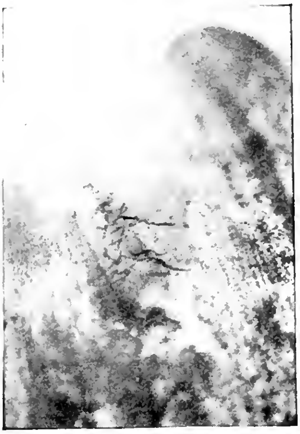

FIG. 1.

FIG. 2.

FIG. 3.

FIG. 4.

some responsibility in the matter although they look to the rail makers for some improvement. I do not think that the rail makers nor the railroads would be satisfied to-day to have half a dozen

Mr. Stevenson. broken rails about which nothing is known taken as a criterion of the cause of rail failures. I have some photographs here that show the relation between slip or skid spots and shelly spots on tires.

FIG. 5

FIG. 6.

FIG. 7.

FIG. 8.

I might call these the genesis of the shell spot. I am sorry these are not lantern slides for then you would be able to see them to much better advantage.

FIG. 9.

FIG. 10.

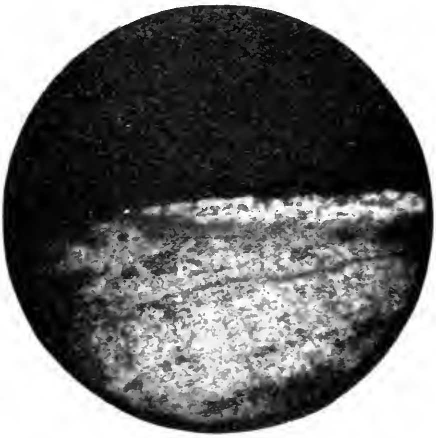

FIG. 11.

Mr. Stevenson. Fig. 1 is a shelly spot on a tender wheel and the evidence of its origin in a slip or skid spot is very plain. There were fully 20 spots similar to this one on the wheel.

Fig. 2 is a hard, slip spot on a coach wheel. There were about fourteen of these spots on the wheel and all too hard to be touched by a tool. Putting the wheel back in service, we found that these spots began to break up into shelly spots.

Fig. 3 is a hard slip spot just about to begin to break up into a shell spot.

Fig. 4 shows two adjacent spots breaking up into shell spots. This shows also how the wheel becomes out of round. The portion of the tread between these two spots being lower than on the opposite sides.

FIG. 12.

Figs. 5, 6, 7 and 8 are from one pair of wheels; Figs. 5, 6 and 7 from one wheel and Fig. 8 from the corresponding position on the other. Here we have an instance where one wheel evidently got all the punishment, and there is no doubt as to the origin of the shell spots in this case.

Fig. 9 shows an etched slip spot on the tread of a tire. The etching has brought out very strongly the outline of the spot and also the incipient shell cracks.

Figs. 10 and 11 are microphotographs of cross-sections through Fig. 9 (magnification 22 diameters) and show the depth to which the hardening action has taken place, and also the distinct line of demarcation between the hardened and unaltered steel.

Fig. 12 is similar to Fig. 9 and shows the incipient shell cracks in the etched slip-spot area. These cracks were not visible to the naked eye until the spot was etched.

Figs. 13 to 18 are slip spots in the early stages of shelling.

Mr. Gibboney. MR. J. H. GIBBONEY.—May I inquire whether or not the heat cracks referred to by the Chairman, developed to a greater extent in the throat of the flange, or was the distribution regular in the

FIG. 13.

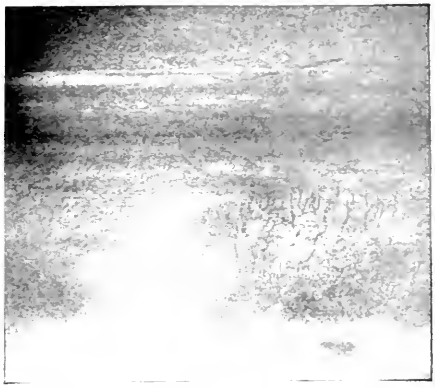

FIG. 14.

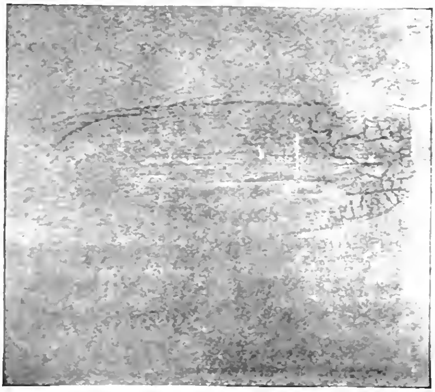

FIG. 15.

FIG. 16.

FIG. 17.

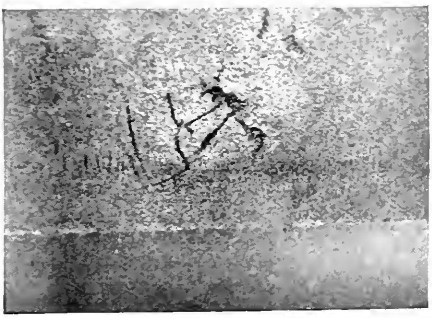

FIG. 18.

Mr. Gibboney. tread? As extensive users of chilled cast iron wheel we have had a number of flange failures, and in nearly every case our examination showed characteristic blue heat seams or cracks in the throat of flange extending in a number of cases through the tread. Chemical examination of the material near the break has, in a great majority of cases, given a very high sulphur content, in some cases running as high as 0.20 per cent. In wheels made at our Roanoke foundry we hold the sulphur at or below 0.10 per cent. and we think that by so doing we have greatly decreased tendencies towards the development of heat cracks. At least the percentage of flange failures in our wheels is by far lower than in wheels purchased from wheel manufacturers. I might add that we have under way a series of tests—chemical and service—to determine the value of vanadium and titanium alloys as purifying agents for cast iron, and we hope to accomplish through the agency of these deoxidizers a much stronger and better locked crystalline structure. I hope to be in position to give to the Society at its next meeting the outcome of these tests.

The Chairman. THE CHAIRMAN.—I would say that the majority of the heat cracks are pretty nearly at what we call the throat of the wheel. We have had them through other parts of the tread somewhat as illustrated. In giving a crude estimate, I would say eighty per cent. of them are at the throat, that is, the junction of the tread and flange.

Very frequently, owing to bad fitting brake shoes, the throat gets as much heat as any other part. The shoe actually digs into the throat.

TESTS OF STAYBOLTS.

By E. L. Hancock.

The tests of staybolts herein reported were undertaken to determine the effect of the speed of vibration on the total number of vibrations required to cause rupture. The specifications for the strength of staybolts, as recommended by our Committee on

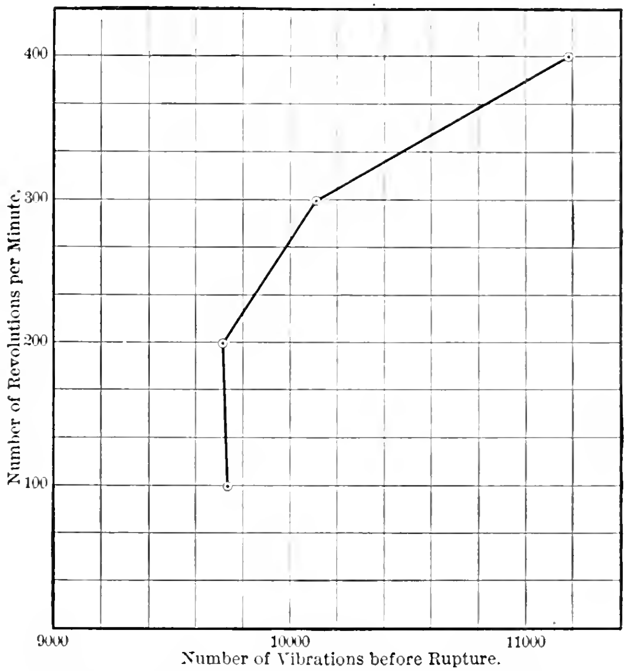

Fig. 1.—Results of Staybolt Tests. Relation of speed of vibration to the total number of vibrations required to produce rupture.

Staybolts last year, contained no mention of the speed of vibration. It occurred to the writer that this point might be worth some study. Inasmuch as one of the committees of the Society had already made tests of the effect of speed on the tensile strength of common

TABLE I.—RESULTS OF TESTS OF STAYBOLTS

Bolt Number.	Number of Vibrations to Rupture.	Speed, R. P. M.
1	10,188	100
2	13,123	100
3	8,339	100
4	9,363	100
5	8,868	100
6	10,880	100
7	11,888	100
8	5,318	100
Average	9,746	100
9	11,748	220
10	14,625	220
11	8,545	210
12	7,251	200
13	11,121	200
14	6,547	200
15	6,535	200
16	9,404	200
17	9,380	204
18	12,154	204
Average	9,731	205
19	8,785	320
20	11,762	300
21	10,763	300
22	9,174	300
Average	10,121	305
23	10,119	370
24	7,063	400
25	11,219	400
26	5,419	400
27	13,265	400
28	13,193	400
29	18,098	400
Average	11,196	396

and staybolt iron, and had found that for commercial purposes the speed is not an important factor, it was thought probable that similar results would be shown in the present case. No proof being at hand, the tests were determined upon.

The material used was one-inch hollow staybolt iron furnished by the Falls Hollow Staybolt Company. This material was threaded with standard staybolt threads, twelve to the inch, and tested in an Olsen vibratory machine, in accordance with the specifications recommended by our Committee on Staybolts last year. This specification requires that "a threaded specimen, fixed at one end, must have the other end moved in a circular path

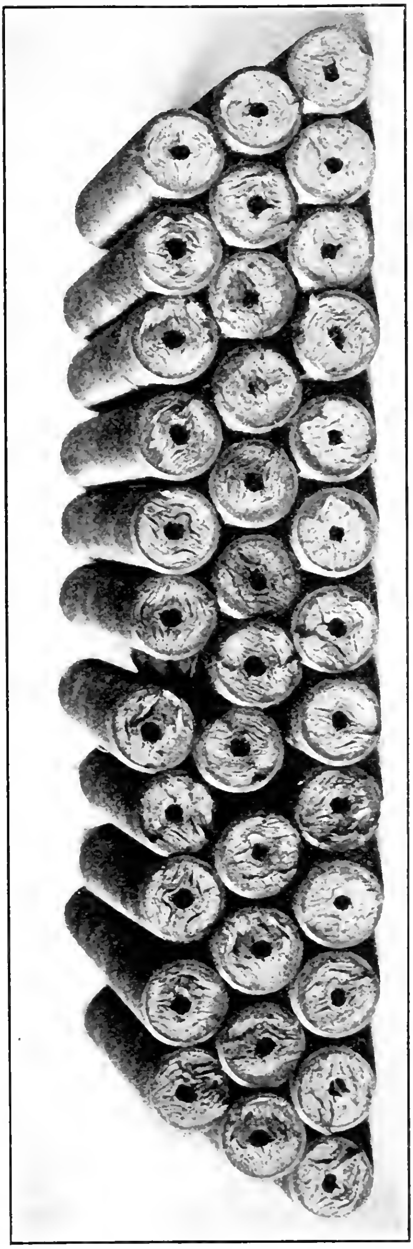

Fig. 2.

while stressed with a tensile load of 4,000 lbs. per sq. in. The circle must have a radius of $\frac{3}{32}$ in. at a point 8 ins. from the end of the specimen." When so tested, the bolt is required to withstand 6,000 revolutions.

The bolts were tested at speeds ranging from 100 to 400 revolutions per minute, while under a tensional stress of 4,000 lbs. per sq. in. The results are shown in Table I.

An examination of the table shows a slight increase in the number of vibrations as the speed of vibration is increased. This is what was to be expected since the tension tests, to which reference has already been made, showed a slight increase in strength with increased speed. The results of the table are shown graphically in Fig. 1. On account of the variation of the results shown for the individual tests and the slight increase in the number of vibrations when the speed changes from 100 to 200 revolutions per minute, it may be concluded from these tests that the speed of vibration is not an important factor and may be neglected. Beyond this speed the number of vibrations increases 3.8 per cent. at 300 revolutions per minute and 14.8 per cent. at 400 revolutions per minute. The character of the fractured bolts is shown in Fig. 2. There was no apparent change in the fracture due to a change of speed.

The tests were all made in the Laboratory for Testing Materials, Purdue University, and the writer wishes to acknowledge the effective services, interest and help of Mr. L. A. Scipio in the conduct of the tests.

RESULTS OF TESTS OF MATERIALS SUBJECTED TO COMBINED STRESSES.

By E. L. Hancock.

During the past four years the writer has conducted tests of steel under combined stresses. These have been carried out in such a way as to combine torsion with tension, flexure and compression. It is the writer's purpose in this brief report to make a summary of these tests, as well as other tests that have been made along the same lines, and to present these results in such form that they may be readily available for the purposes of the engineer.

Among designers of structures subjected to combined stresses three theories have found favor, namely:

1. *The Maximum Stress Theory.*—This theory assumes that the material yields when the tensile (or compressive) stress on any element reaches a certain maximum. It leads to the following relation:

$$q=\frac{1}{2}\left(p \pm \sqrt{p^2+4p_s^2}\right) \tag{1}$$

where q is the greatest unit tension (or compression) on any element and p and p_s are the unit tension (or compression) and unit shear respectively, found by testing the material in simple tension (or compression) and simple shear. This theory has been used almost exclusively by American and English engineers.

2. *The Maximum Strain Theory.*—This theory holds that the material yields when the deformation of any element in tension or compression reaches a certain amount. It holds that Poisson's ratio (the ratio of lateral contraction to longitudinal deformation) is 0.25. Using this ratio the following formula results:

$$q=\frac{3}{8}p \pm \frac{5}{8}\sqrt{p^2+4p_s^2} \tag{2}$$

where the symbols have the same meaning as before. The values

of Poisson's ratio for various materials as determined from tests are:

Steel, hard	0.295
Steel, soft	0.299
Iron	0.277
Brass	0.357
Lead	0.375

It will be seen that these values differ considerably from the value 0.25, so that the formula can hardly be expected to furnish correct results. This formula is in use to a considerable extent in continental Europe.

3. *The Maximum Shear Theory.*—The third theory, and one but little used, is that the material yields when the shear on any element reaches a maximum. This leads to the relation:

$$q_s = \frac{1}{2}\sqrt{p^2 + 4p_s^2} \qquad (3)$$

where q_s is the greatest unit shear on any element. It will be seen from Fig. 1 and Fig. 2 that this formula fits the results of all the tests much more exactly than either of the others.

Tests made by the writer,* by Mr. J. J. Guest † and by Mr. W. A. Scoble ‡ have been used in making Fig. 1. The tests made by Mr. Guest and Mr. Scoble were made by taking the yield point as a basis, while those made by the writer were made by taking the elastic limit as a basis. Examination of Fig. 1 shows very little difference as to which is taken as a basis. The convenience of locating the yield point suggests an advantage in its favor.

It is seen that the curve is an ellipse with the unit tensile (or compressive) stress as one semi-axis and the unit shear as the other semi-axis. If the shearing strength equals the tensile strength, the curve becomes a circle; this, however, is not generally the case. The equation of the ellipse may be derived as follows:

* "Effect of Combined Stresses on the Elastic Properties of Steel." E. L. Hancock, *Am. Soc. Test. Mat.*, Vol. V, 1905; Vol. VI, 1906; Vol. VII, 1907. Also *Phil. Mag.*, Feb. and Oct., 1906, and Feb., 1908.

† J. J. Guest, *Physical Soc. of London*, Sept., 1900.

‡ W. A. Scoble, *Phil. Mag.*, Dec., 1906.

Let p be the fiber stress in tension or compression.
Let p_s be the fiber stress in shear.
Let b be the fiber stress at the elastic limit in tension.
Let a be the fiber stress at the elastic limit in shear.
Let M_1 be the bending moment in flexure.
Let M_2 be the twisting moment.

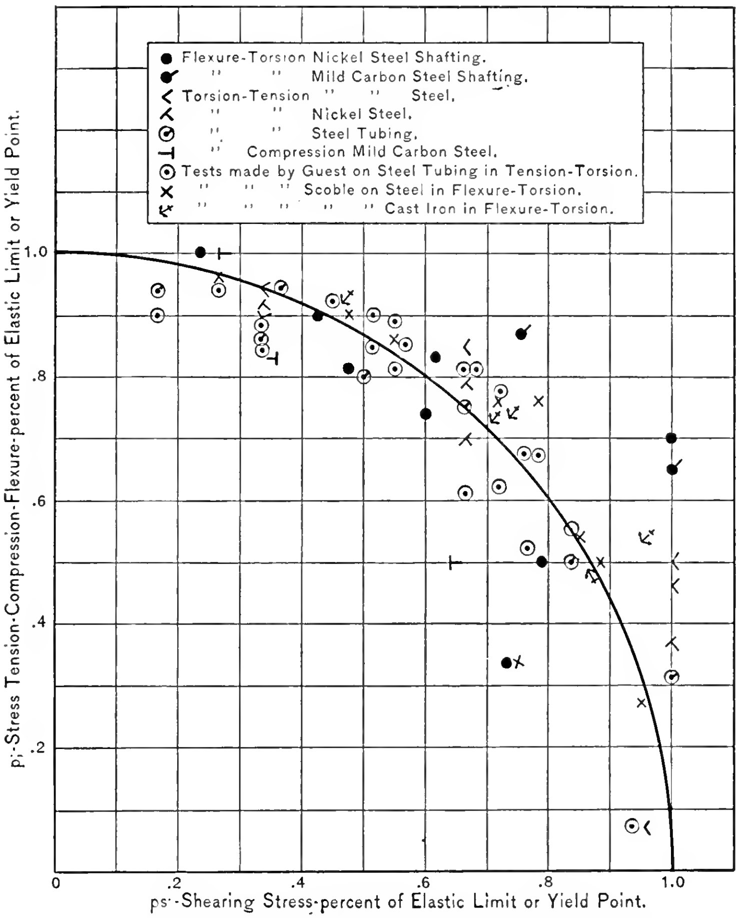

FIG. 1.—Results of Tests of Materials under Combined Stresses.

Then, from the curve

$$\frac{p_s^2}{a^2} + \frac{p^2}{b^2} = 1 \tag{4}$$

Knowing a and b from the results of tests of the material in simple tension and torsion and either p or p_s, the other may be determined.

The equivalent twisting moment for a shaft subjected to combined bending and twisting, derived from (4), is given by the relation

$$M_e^2 = M_2^2 + \left(\frac{4a^2}{b^2}\right) M_1^2 \qquad (5)$$

where M_e is the equivalent twisting moment.

If the elastic limit in shear be taken as one-half of the elastic limit in tension, (5) becomes

$$M_e^2 = M_2^2 + M_1^2 \qquad (6)$$

which, lacking a constant factor, is the formula derived by considering the maximum shear theory to hold. This assumption that a equals one-half of b is hardly borne out by the results of tests that have been made, as is shown by Table I.

TABLE I.—RESULTS OF TESTS.

Material.	b Fiber stress at elastic limit, tension, lbs. per sq. in.	a Fiber stress at elastic limit, shear, lbs. per sq. in.	$\frac{b}{2}$	$a \lesseqgtr \frac{b}{2}$	Value of $\frac{4a^2}{b^2}$
Steel tubing	21,000	10,500	10,500	$=$	1.00
Nickel steel	76,500	38,000	38,200	$<$	.98
Mild carbon steel.	47,000	30,400	23,300	$>$	1.66
Steel (Scoble)	64,600	29,170	32,300	$<$	.81
Carbon steel.....	35,500	24,400	17,700	$>$	1.84
Rivet steel	38,900	23,400	19,400	$>$	1.44
Nickel steel	56,000	36,000	28,000	$>$	1.66
Steel tubing	17,000	11,500	8,500	$>$	1.82
Steel tubing	28,000	16,000	14,000	$>$	1.30
Steel tubing	20,000	12,000	10,000	$>$	1.44

An attempt has been made in Fig. 2 to show the difference in the shearing strength given by the formulas representing Theories 1, 2 and 3 and expressed in Equations (1), (2) and (5). The material under consideration had an elastic limit in tension of 76,500 lbs. per sq. in. and an elastic limit in torsion of 38,000 lbs. per sq. in. Equation (5), the equation of the curve in Fig. 1, gives the proper elastic limit in shear (the elastic limit in tension

was assigned in all cases), while (1) and (2) give an elastic limit much too large. Nearly all values in shear given by (1) and (2) are larger than those obtained from tests. This means that the corresponding bending or twisting moment will be too small and if used in design may lead to disaster.

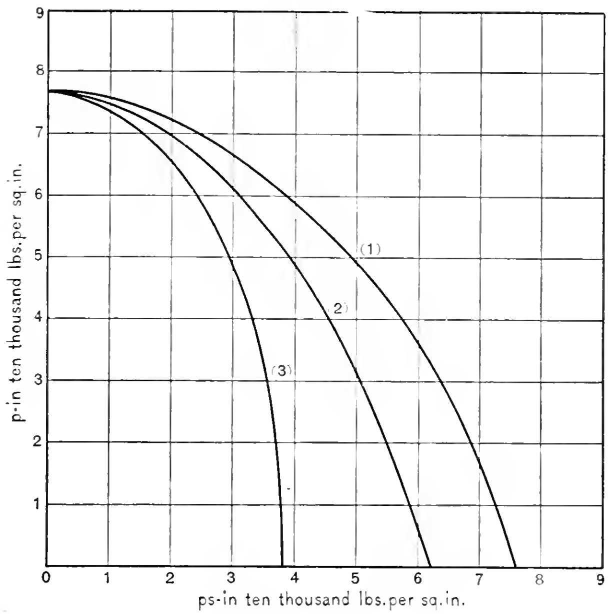

FIG. 2.—Curves Showing Shear as Given by the Formulas:

(1) Maximum Stress Theory

$$q = \tfrac{1}{2}\left(p \pm \sqrt{p^2 + 4p_s^2}\right)$$

(2) Maximum Strain Theory

$$q = \tfrac{3}{8}\,p \pm \tfrac{5}{8}\sqrt{p^2 + 4p_s^2}$$

(3) Maximum Shear Theory

$$q_s = \sqrt{p^2 + \frac{b^2}{a^2}\,p_s^2}$$

In conclusion, it may be said that both the maximum stress theory and the maximum strain theory give an equivalent moment that is too small. They should not, therefore, be used without a large factor of safety. The maximum shear theory agrees very

nearly with the results of tests and the resulting formula, or some slight modification of it should be used in practice. The following formulas are recommended:

$$q_s = \sqrt{p^2 + \frac{b^2}{a^2} p_s^2}$$

$$M_e^2 = M_2^2 + \frac{4a^2}{b^2} M_1^2$$

CHARACTERISTIC RESULTS OF ENDURANCE TESTS ON WROUGHT IRON, STEEL AND ALLOYS.

By Henry Souther.

Since the last meeting the writer has continued alternate stress endurance tests with the White-Souther machine and with alloys not ordinarily submitted to such a test.

The automobile industry is making use of what may be called new alloys, principally of aluminum. The results are interesting, and an endurance test is pertinent because failures in automobiles are fatigue failures due to vibration or alternate stress.

Before touching on the details of the test, I wish to call attention to a detail of the testing machine that has been changed during the past year. This change is valuable not only in connection with the testing machine itself, but as showing what an important thing a slight angularity in a shaft may be under certain conditions.

It must be understood that the specimen is supported in the middle and loaded at both ends (Fig. 1), and that the loads are carried on ball bearings which push snugly over the ends of the specimen against a collar. When the load is applied this specimen bends to some extent. The amount of deflection for steel, for example, is often 0.06 in. or 0.07 in., the length of the deflected portion being about 4.50 ins.

Referring to Fig. 2, it is seen that the weight-supporting rods E are attached to the ball-bearing case D at a point about 1.6 ins. below the neutral axis of the specimen. The line of pull, because of the deflection, is therefore not in the plane of the balls and ball bearing by an angle of about one degree. This angularity, coupled with the fact that the point of support of the load is considerably below the neutral axis, sets up a cramping action throughout the bearing itself and at the point of contact between the bearing and the specimen.

This angularity seems like a trifling matter, especially with a ball bearing apparently more than ample in size for the loads

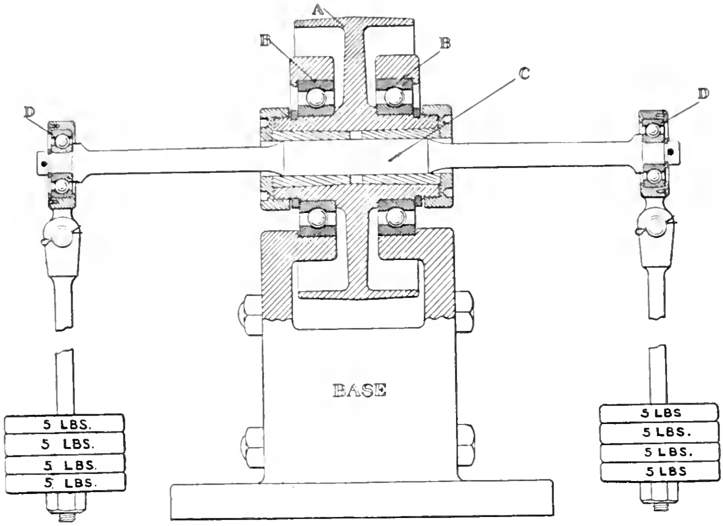
A
B
B
C
D
D
BASE
5 LBS.
5 LBS.
5 LBS.
5 LBS.
5 LBS
5 LBS.
5 LBS.
5 LBS

FIG. 1.

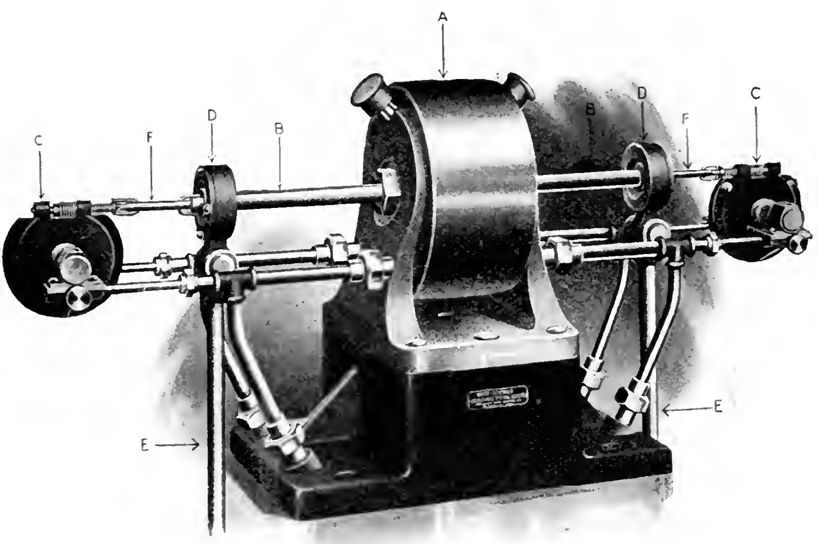
A
C
F
D
B
D
F
C
E
E

FIG. 2.

placed upon it. Nevertheless, these ball bearings failed repeatedly in an unexpected manner, and the snug fit of the ball bearing on the specimen wore and loosened badly because of the cramping action referred to.

It was suggested that the point of support of the load be changed from a point below the neutral axis of the specimen to the plane of the neutral axis of the specimen. The construction is now as shown in Fig. 3. It will be seen that there is a pair of gimbals at each side of the ball case and that the load hangs from these. Although the line of pull is not in the plane of the balls of the bearing, the performance is good, because there is no leverage and no resulting cramping action. Consequently, the slip-fit between the bearing and the specimen is no longer a source of trouble and the ball bearings endure.

This same cramping action affected the tests to some extent. The bearing necessarily changed its point of contact with the specimen when a little worn. The constant creeping of the bearing caused a certain movement in the weights. Both actions had some effect on the fiber stress in the specimen and introduced vibrating strains which it was impossible to regulate.

Wrought iron survived as the best material for axles for many years in its competition with steel for a similar purpose. It still survives in some localities to a small extent. I felt that it would be interesting to investigate the endurance of wrought iron in this machine and consequently obtained a specimen. After getting the physical characteristics of it in tension test, I then sought a piece of steel of as near the same physical characteristics as possible. F. D. Carney, of the Pennsylvania Steel Company, succeeded in getting a specimen that compared very closely as far as elastic limit and tensile strength was concerned. The results obtained on both specimens are given in Table I, as well as the analyses and physical characteristics.

The specimens were loaded with identical loads, that is, a load that would produce a fiber stress equal to about two-thirds of the elastic limit in tension.

It will be seen that the endurance of wrought iron under these conditions was twenty million revolutions, and that of the basic open-hearth steel only 1,891,000 revolutions.

It must be remembered in examining these results that the

TABLE I.—COMPARATIVE ENDURANCE TEST OF WROUGHT IRON AND STEEL.

Chemical Analysis;

Mark.	Material.	C, per cent.	P, per cent.	Mn, per cent.	S, per cent.	Si, per cent.
42880	Wrought iron	0.03±	0.328	0.04	0.044	0.19
43338	Basic steel	0.07	0.012	0.52	0.020	0.02±

Tensile Test:

Mark.	Material.	Elast. Limit, lbs. per sq. in.	Ult. Strength, lbs. per sq. in.	Reduct. of Area, per cent.	Elongation in 2 ins., per cent.
42880	Wrought iron	37,400	53,000	35.0	24.0
43338	Basic steel	36,000	56,500	64.5	40.0

Endurance Test:

Mark.	Material.	Load, lbs.	Fiber Stress, lbs. per sq. in.	Endurance, revolutions, No. 1 end.	No. 2 end.
42880	Wrought iron	80	28,270	20,882,800†	17,556,200*
43338	Basic steel	80	28,270	835,300*	1,891,300*

* Point of failure.

† On an increase of load to 110 lbs., corresponding to a fiber stress of 38,800 lbs. per sq. in., failure took place after 286,000 revolutions.

basic steel that I obtained to duplicate the wrought iron is considerably softer than any used for structural purposes, except for rivets or fire-box flange steel. I doubt if so soft a basic steel ever went into axles. It is altogether too difficult to machine in the first place and in the second place is too weak, as compared with other steel equally available and low in price. It is, therefore, apparent that for a given elastic limit, wrought iron endures better under alternate stress than steel. It would be interesting to continue this test further with wrought iron as a basis of comparison and find out how good a carbon steel would be required to endure under these conditions with the wrought-iron standard.

It is not often, perhaps, that cast brass or bronze is submitted to alternate stress, but it certainly is in the engine base of an automobile engine, or possibly to a less degree in the gear case. At any rate, such material has broken under fatigue strains. Consequently, I felt that endurance tests would be illuminating and obtained some ordinary bronze specimens.

It will be noticed that the tension results (see Table II) show a tremendous variation in the cast metal, and I may say that this is characteristic of the cast brass or bronze specimens that come

into my laboratory. They are not dependable as to soundness and physical characteristics as a rule.

These specimens were loaded a little more heavily than for

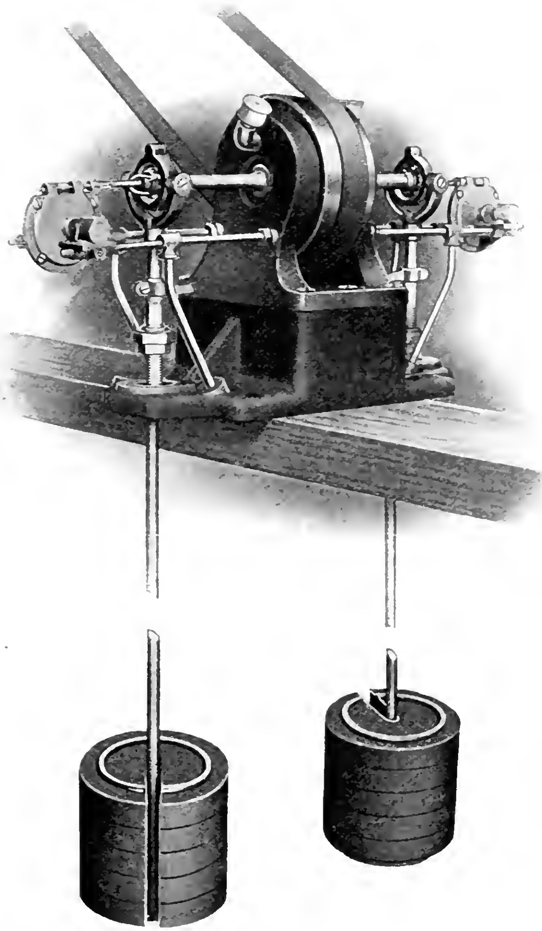

FIG. 3.

aluminum, namely, 35 lbs. actual load, producing a fiber stress of 12,500 lbs. per sq. in. The widest variation in results is observable. It also appears that the only tension specimen that

TABLE II.—ENDURANCE TESTS OF CAST BRONZE.

Composition

Mark.	Copper, per cent.	Tin, per cent.	Lead, per cent.	Zinc, per cent.	Phosphorus, per cent.
43265	87.28	9.94	0.38	2.40	?
43266	87.49	9.94	0.34	2.23	?
43267	86.06	11.26	0.02	2.66	?
43268	79.30	9.31	9.87	0.91	0.61

Tensile Test:

Mark.	Elast. Limit, lbs. per sq. in.	Ult. Strength, lbs. per sq. in.	Reduction of Area, per cent.	Elongation in 2 ins., per cent.
43265	?	18,500	..	...
43266	?	14,500	..	...
43267	26,000	29,000	11	8.0
43268	....	21,000	..	...

Endurance Test:

Mark.	Load, lbs.	Fiber Stress, lbs. per sq. in.	Endurance, revolutions. No. 1 end.	No. 2 end.
43265	35	12,400	378,700*	2,436,700*
43266	35	12,400	761,100*	23,800*
43267	35	12,400	10,000,000†	3,873,000*
43268	35	12,400	862,500*	483,200*

* Point of failure.

† On an increase of load to 50 lbs., corresponding to a fiber stress of 17,700 lbs. per sq. in., failure took place after 224,600 revolutions.

gave good physical characteristics, with elongation and reduction of area, was the only specimen that endured under alternate stress a very long time; in fact one end of this specimen had to be further loaded to produce a fiber stress of 17,600 lbs. per sq. in. in order to cause rupture within a reasonable length of time.

The controversy between the aluminum-zinc alloy adherents and "anti-zinc" alloy adherents is still as lively as ever. West of Buffalo and Pittsburg zinc in aluminum castings is not countenanced for automobile construction. Reputable makers regard it as positively useless. East of the two cities, as a rule, aluminum alloys containing zinc are used.

The zinc alloys really have the best physical characteristics under tension test, and under endurance test the results given in Table III also show superiority for the zinc alloy.

Zinc, or spelter, as it is purchased, is very coarsely crystalline as everyone well knows. It is not unlikely that the prejudice against zinc as an alloying element exists because zinc is known

TABLE III.—COMPARATIVE ENDURANCE TEST OF ZINC AND NON-ZINC ALLOYS.

Composition, etc.:

Mark.	Material.	Specific Gravity.	Manganese, per cent.	Copper, per cent.	Aluminum, per cent.	Zinc, per cent.
40330	Non-zinc alloy	2.88	0.02	8.86	91.12	0.0
42401	Zinc alloy	3.05	...	3.0	67.40	29.6

Endurance Test:

Mark.	Material.	Load, lbs.	Fiber Stress, lbs. per sq. in.	Endurance, revolutions. No. 1 end.	Endurance, revolutions. No. 2 end.	Deflect. at end of $4\frac{1}{2}$-in. specimen ins.
40330	Non-zinc alloy	30	10,600	Broke immediately	466,900	0.06
42401	Zinc alloy	30	10,600	6,467,100	8,815,700	0.04

to be crystalline when by itself, and therefore likely to crystallize easily, as the expression goes, in a casting.

This is another good example of the impossibility of prediction with alloyed metals. A pure metal that may be crystalline in its unalloyed condition may easily assist in producing the finest kind of grain when properly alloyed.

The influence of vanadium in steel must be further studied to be fully understood. At the present moment it would appear from the experiments made in my laboratory that a higher heat of treatment is necessary to bring out the best qualities of vanadium steel than with any other steel treated. Nickel and nickel-chrome in the absence of vanadium respond to quenching and annealing treatments at about 1500° F. Although I am not willing to fully commit myself, because of the relatively few specimens tested, it would seem now that vanadium steel and particularly chrome-vanadium steel requires a temperature approaching 1700° F. for good results.

The writer hopes that something of value may develop in the discussion, or that investigators may take up this subject. I still doubt the probability of this characteristic, because for metallurgical reasons it would seem that there is no reason why vanadium should prevent the response of any alloy steel to heat treatment at customary temperatures; that is, temperatures only a little above the point of recalescence of the steel.

METHOD OF OBTAINING A TRULY CIRCULAR AND UNIFORM CHILL IN ROLLS.

By Thomas D. West.

The determination by actual test of the difference in strength of turned chill and sand-rolls of like diameters, both cast from the same ladle of metal, would be very valuable to engineers and manufacturers. So far as the writer is aware, no such tests have been made. It is reasonable to expect, however, that the strength of the sand-cast roll will exceed that of the chill roll, if both be cast from the same metal, especially if there should be any variation in the thickness of the chill. This view will doubtless be shared by those who have had extended experience in the use of chill rolls.

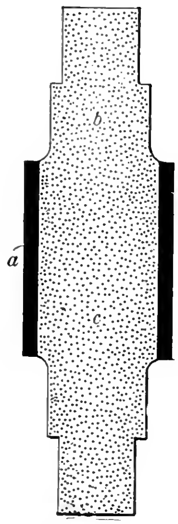

Fig. 1.

In casting a chill roll the general conditions are as shown in Fig. 1. The metal in the outer part *a* (shown in black) will present a white appearance for a depth of $\frac{1}{4}$ to 2 ins. or more, while that in the necks and body, *b* and *c*, will be mottled or nearly gray. In *a* the carbon exists in the combined form, while in *b* and *c* it is largely graphitic. The fact that the shrinkage of the former in solidifying is considerably greater than that of the latter is an important consideration in the manufacture of rolls. If two test-bars, under 2 ins. square in section, be cast from the same ladle, the one in an iron, the other in a sand mold, the contraction of the former will be about twice that of the latter. This is shown by tests made by the author, treated more fully in his "Metallurgy of Cast Iron" (pp. 413-15).

When a chill roll mold is being filled with metal the cooling qualities of the chiller *d*, Fig. 2, cause the outer body of the inflowing metal, as it comes in contact with the chilled face *e*, to solidify

so rapidly that a contracting crust is formed in two to five minutes, according to the diameter of the roll.

The formation of the sustaining crust is easily explained as follows: On the side *e*, Fig. 2, the metal is shown as it fills the mold, while on the opposite side *f* the conditions that exist two to five minutes after the roll is cast are indicated. It is seen that the chiller *d* has ceased to offer any support to the crust *a* by reason of the space created at *x* and that the chiller *d* might be removed, so far as the body of the roll is concerned, if that were practicable. It is also evident that the metal inside the crust *a* and the necks of the rolls is still in almost as hot and liquid a condition as when first poured. Its fluidity may, in fact, be maintained for 20 to 60 or more minutes, according to the size of the roll. This condition serves to explain in large part the difficulty of obtaining satisfactory chill rolls. The thin, almost semi-molten, crust *a* must sustain, during the early period of the operation, the static pressure of the confined liquid metal. The result in obtaining a chill roll casting is affected by small variations in the level of the mold, the quality of the metal, the temperature in pouring, the catching of fins at joints or crevices in the face of the chiller, the operation of feeding devices, etc. It is no wonder, therefore, that with a process so sensitive and difficult to regulate, the losses range from 10 to 30 per cent. of the nominal output by the systems now in use, and that the rolls cast in this manner are rarely truly circular with a uniform thickness of chill.

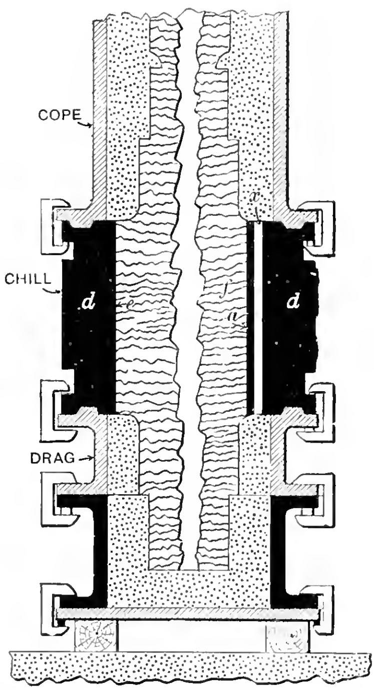

Fig. 2

Again, a buyer who accepts a roll of uneven thickness of chill and not truly round runs great risk of breakage. It is certain, at least, that such rolls cannot stand the rough usage and wear that may be expected of truly circular ones with a uniform thickness of chill.

For the successful casting of chill rolls the appliance should be such as to enable the chiller *d* to remain constantly in intimate contact with the crust *a*, and that it be controllable likewise vertically to take care of the contraction in that direction. Numerous devices designed to overcome the evil effects of a contracting crust have been tried, but it is believed that none of these meet the requirements in anything like the same measure as the appliance represented in Fig. 3, which differs radically from anything previously attempted, and is the invention of the author's son, Mr. Ralph H. West, who has had considerable experience in the manufacture of chill rolls.

In the use of this device the action of the contracting crust was demonstrated in a way never done before, and it served to show what may be accomplished to insure a truly circular roll casting of uniform thickness of chill. To make this clear a detailed description of the operation will first be presented.

The chiller *d*, Fig. 3, has grooves *i*, which are shown in an exaggerated form in the illustration. These grooves vary in height and number to suit varying requirements, and exhibit gradual variations from bottom to top in the inclination of the sloping surfaces *h* and *j* for the purpose of taking care of the vertical contraction of the solidifying crust. This serves to insure a close contact between the upper slope of every groove and the corresponding bead cast on the chill roll during the entire period of settlement to final rest, which is brought about in the following manner.

After the metal is poured through the swirl gate *k*, the band *l* operated by a lever, not shown, is moved, which serves to bring holes in the band to positions opposite *m*, this action allowing fine sand or other flowable material to run out of the partitions *n* and *p* at any desired speed. The chiller *d* is lowered by this action, causing the sloping surfaces *h* and *j* to remain in intimate contact with the upper sloping surfaces of the beads cast on the chilled surfaces of the roll.

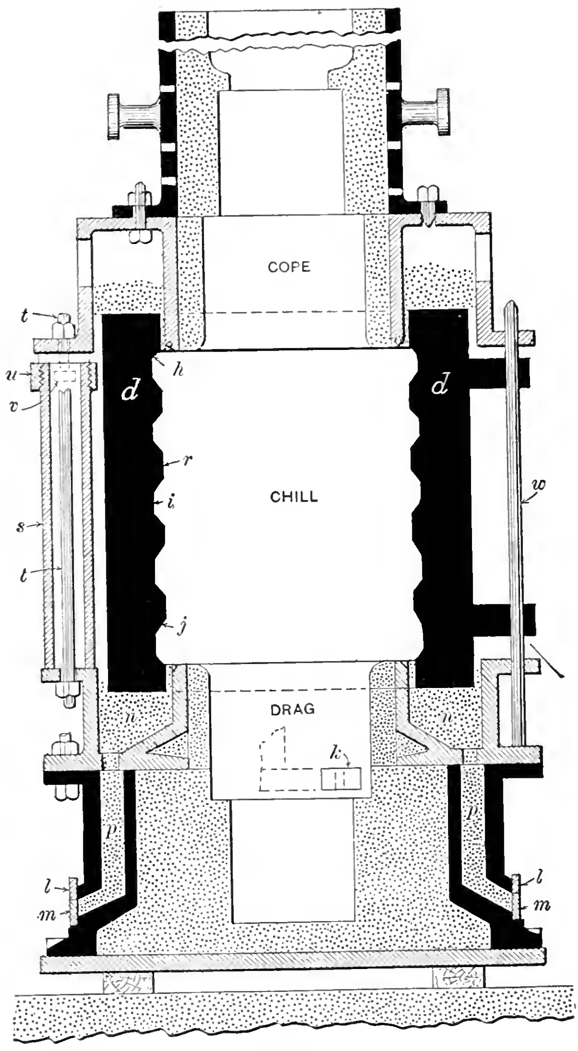

FIG. 3.

In the first roll cast by this device the chiller commenced to settle in about two minutes after the pouring was finished, and in about seven minutes the grooved face *r* had slid over the bead face *i* thus giving direct indication of the internal action of the contracting chill crust, a matter never before accomplished.

In this device the outer diameter at the points *i* is such that when the roll is cold enough to be removed from the chiller it can be hoisted out of the latter, or the latter off the roll, as may be desired. The body of the sample roll cast in this chiller was 11 ins. in diameter and 14 ins. long. When put in the lathe to be turned it was found to be perfectly round and of an exact thickness of chill at all points of its circumference.

Five other devices differing in the form of the grooves and the method of insuring contact have been designed. In one case the chiller remains stationary while the cope, casting and drag can be lowered. In another the crust is depressed. In a third the cope and casting settle into the drag, while the chiller remains stationary. In some of these devices the grooves, or hugging faces, are so arranged that they remain in constant contact with the beads on the roll until the latter is removed from the chiller or its molds.

Roll turners claim that the extra labor in turning up chill rolls to remove the slight projections or beads cast on the rolls by this system is, on the average, much less expensive than that required to turn up smooth-faced chill rolls that are not truly circular, manufactured by processes now in use in our chill roll foundries.

Returning to Fig. 3, the cope is supported from the drag independent of the chiller by means of four wrought iron tubes *s*, bolts *t*, adjustable sleeves *u* and set screws *v*. Three or four long turned rods or pins *w* serve to center the chiller and cope with the drag.

It is believed that with the above explanation those familiar with founding will readily understand the operation of the device and appreciate its utility.

MANGANESE BRONZE.

By C. R. Spare.

It is not the writer's intention to review in this paper the history of the development of the various manganese bronzes up to the present day, but to outline briefly the vastly extended uses to which these valuable alloys have been applied in recent years by the various industries and, more especially, to point out the methods of testing employed and the physical results obtained.

The best present-day alloys of manganese bronze are the results of years of scientific research by several of the largest manufacturing concerns in the world, who maintain chemical and physical testing laboratories in connection with their brass foundries. Of all the metal industries, brass founding is almost the last to pass from the empirical rule-of-thumb to a scientific metallurgical basis. In fact the majority of the brass and bronze manufacturers to-day have never used chemical analysis or a testing machine and have therefore only a general idea of the composition and physical properties of their products. This is the result of natural conditions, which years ago did not impose severe requirements on brass and bronze.

To-day conditions have changed, or are changing, and engineers have multiplied powers and manipulated pressures beyond all former figures. Machinery is more powerful, more complicated and more severely tested than ever before, and while steel and other industries have made great advances and kept apace of requirements, the same ratio of improvement has not been maintained in the copper and brass metals, except in the case of the high tensile strength manganese bronzes.

There are several brands of manganese bronze on the market, each with its own characteristics, determined by the chemical composition and the process of manufacture. While upon analysis some of these bronzes do not reveal striking differences in composition, yet these alloys are susceptible of wide variations in physical properties by slight changes in composition, difference in quality of raw materials and methods of melting and mixing.

Certain impurities affect very injuriously the strength and ductility of manganese bronze, causing brittleness and crystalline texture. It is not within the compass of this paper to discuss these differences but they become very evident in the testing machine.

Manganese bronze found its first important practical application in the manufacture of propeller wheels for ships. Now, cast iron and cast steel have been almost universally displaced by it; not only all naval vessels but the merchant ships of the world have adopted it as standard practice. Marine engines have been increased to such vast powers, especially during the past ten years, and since the advent of the steam turbine, that 20,000, then 40,000 and now 70,000 I. H. P. are transmitted through manganese bronze propeller wheels. In some ships the speed runs upwards of 300 revolutions per minute.

There is no other metal of equal strength and toughness which will produce such sound, smooth and intricate castings, true to the form of the pattern. These qualities permit the maximum fineness of section of the propeller blade, and at surface speeds of thousands of feet per minute the surface friction is reduced to a minimum.

This bronze is practically incorrodible in sea and alkali waters. Likewise dilute acids and acid mine-water are withstood very successfully. Long-time tests have been made with the various acids and mine-waters. Cast blocks of manganese bronze immersed in the most acid mine-water in Pennsylvania, containing about 300 grains of sulphuric acid per gallon, for one year, showed no material corrosion, and a test-piece cut from this metal showed no diminution in tensile strength over a duplicate specimen previously tested.

Test-pieces cut from cast propellers should show an average ultimate tensile strength of 70,000 lbs. per sq. in., an elastic limit of about 35,000 lbs. per sq. in., or one-half the ultimate strength, elongation in 2 ins., 25 per cent., and reduction of area, 25 per cent. The reduction of area follows the elongation closely in cast manganese bronze.

These figures can be varied to suit requirements. A soft manganese bronze having an ultimate strength of 60,000 lbs. per sq. in. and 40 or 50 per cent. elongation in 2 ins. is the lower limit, while an exceedingly hard manganese bronze can be made to test over 90,000 lbs. per sq. in., with even as much as 30

per cent. elongation. The methods of testing manganese bronze physically do not differ materially from the standard methods for steel. Test-pieces according to the standard United States Government sizes, .505 in. .798 in., or 1.000 in. in diameter are machined 2 ins. between punch marks, and threaded. In the case of castings there is not a sharply defined elastic limit and the yield point cannot be determined by the drop of the beam. Multiplying dividers (such as devised by J. A. Capp), or a standard extensometer will establish quite closely the point at which an appreciable change in the rate of stretch takes place. In the case of rolled or forged manganese bronze, the yield point is more closely defined and the elastic curve is frequently sharp enough to cause the drop of the beam or a halt in the gauge wheel of the testing machine.

In compression, cast manganese bronze, if properly made, shows an average elastic limit of 35,000 to 40,000 lbs. per sq. in., and a maximum crushing load of 90,000 to 100,000 lbs. per sq. in. Rolled or forged manganese bronze shows from 50,000 to 60,000 lbs. elastic limit and as high as 130,000 to 150,000 lbs. maximum crushing load.

Manganese bronze can be rolled or forged readily at a red heat with the production of an exceedingly tough, dense, and close-grained metal. Microscopic examination of cast manganese bronze, after polishing and etching, reveals a very homogeneous and uniformily-grained metal, but after rolling or forging to a sufficient reduction, the structure is reduced to from $\frac{1}{30}$ to $\frac{1}{50}$.

Rolling or forging raises the proportional elastic limit to from 45,000 to 75,000 lbs. per sq. in. depending upon the finishing temperature and the amount of work done on the metal. Likewise the ductility and toughness are increased, without, however, a corresponding increase in ultimate strength.

Forged and rolled rods find a wide application as piston rods, shafts, axles, and for all purposes where a metal of equal strength and toughness to carbon steel is desired, which will not rust or corrode in the atmosphere, mine or sea water.

An especially soft and tough metal is made to resist vibratory and sudden stresses and shocks. It is used under very severe conditions in modern naval ordnance. It also is finding application in staybolts for locomotives. This bronze shows 40 to 50 per cent. elongation, with about 60 per cent. reduction of area.

Thus only a few of the better known applications of manganese bronze have been pointed out. The electrical industry is calling for it for turbo-generator sets which are run at speeds up to 4,000 revolutions per minute, and the blades of the steam turbines are satisfactorily made of extruded manganese bronze shapes. In this case the erosive action of high-pressure steam is a severe condition, under which most metals fail.

The infant industry of this country, which has grown to such lusty proportions, the automobile, has set a very high standard for materials which must stand up under its peculiarly hard service conditions. Automobiles going over all kinds of roadbeds at 60 and 100 miles an hour, with gasoline motors of 60 and even as high as 130 H. P., running at speeds of 1,000 to 1,500 revolutions per minute, have stimulated the improvement of the materials entering into their construction more, perhaps, than any other modern machines. The nickel-chrome and other high grade steels in gears and drop forgings have produced results never heard of before, and cast iron has been wonderfully improved by the requirements of automobile cylinders. Likewise manganese bronze, formerly used almost exclusively in warships, has been adapted to produce forgings and castings which will not crystallize or fail under similarly severe conditions.

As stated at the outset, it was not the purpose of this paper to explain the theory or history of the deoxidation of copper by means of manganese, but an attempt has been made to outline the growing application of these alloys and to present a general idea of the physical tests obtained by several Government and private testing laboratories, with the hope that the engineers of this country will become more interested in and better acquainted with these alloys.

DISCUSSION

The President.—Could the author give us a typical analysis of manganese bronze, that is, approximately the proportions of its constituents? The President.

Mr. C. R. Spare.—The average copper content is about 56 or 57 per cent., zinc 38 to 40 per cent., tin about one per cent. and iron $1\frac{1}{2}$ per cent. The manganese, or course, is largely fluxed off in the deoxidation process. Mr. Spare.

Mr. T. D. Lynch.—I should like to ask Mr. Spare if he uses the same analysis in his casting bronze as in his forging bronze. My impression is that one cannot forge casting bronze, nor make a casting from a forging bronze. Mr. Lynch.

Mr. Spare.—That is correct. The metal in the two cases is decidedly different. You cannot use the same formula for bronze forgings as for castings. Mr. Spare.

The President.—I may say that we have tested some of this metal at Altoona, and while it has not gone into very general use we have been quite struck with its interesting properties. In London some years ago this metal was known as Dick's Delta metal; and Mr. Dick explained to me that the metal was made by adding iron borings to melted zinc, either cast iron or chips from shops or old waste material, until the zinc was saturated. Zinc dissolves iron and manganese, and as it dissolves the melting point of the alloy increases. The saturation point is about 9 per cent. of iron, or iron and manganese, or manganese, the balance being zinc. Then this alloy is melted with copper to form the manganese bronze. I do not know whether that is the process used in the present instance. I do know, however, that this alloy of zinc and copper and iron and more or less manganese has the most remarkable physical properties. You can forge it and cast it, and, if I remember rightly, it has as high as 70,000 pounds tensile strength. The President.

Mr. Spare.—The tensile strength is about 90,000 pounds. Mr. Spare.

Mr. G. H. Clamer.—The name "manganese bronze" is really a misnomer. Manganese is only used as a fluxing agent in the standard brand of manganese bronze on the market. But the Mr. Clamer.

Mr. Clamer. best results I have ever been able to obtain were by the actual incorporation in the alloy of several per cent. of manganese. In using manganese care must be taken not to have any carbon present; and therefore the manganese copper that is produced in the electric furnace, made from manganese dioxide or pyrolusite, produces the best results. By using an alloy of $3\frac{3}{4}$ per cent. of manganese, the balance being approximately of the analysis that Mr. Spare gives, we have gotten a tensile strength a little above 90,000 pounds with an elastic limit of about 42,000 to 43,000 pounds. Of course the use of manganese copper made in that way makes the alloy very expensive. In fact, manganese costs about a dollar a pound, and can only be used on special occasions. The ordinary manganese bronze is made in accordance with the formula which Mr. Spare gives. I don't think he mentioned the fact that it contains a little aluminum. In casting manganese bronze I understand it must contain a little aluminum, and manganese bronze to be forged must not contain that metal. The uses to which manganese bronze is being put to-day are very extensive. It is coming very largely into use in the automobile industry, and the government is using large quantities of it. A great many manufacturers are becoming interested in the production of manganese bronze, and I think that in the near future its use will be greatly extended.

NOTES ON THE DESIRABILITY OF STANDARD SPECIFICATIONS FOR HARD-DRAWN COPPER WIRE.

By J. A. Capp and W. H. Bassett.

Until within the last few years, hard-drawn copper wire has been used principally for telephone and trolley lines. With the increasing development of hydro-electric plants where the generating stations are apt to be at considerable distances from the point of application of the power, there has been a widely-growing use of hard-drawn copper wire for the transmission lines; and the time seems ripe for the preparation of specifications for hard-drawn copper wire which shall be fair and reasonable from the standpoints both of the manufacturer and of the consumer.

The specifications already in existence agree only in that they all require certain minimum conductivity and tensile strength, and that they permit certain variations from nominal diameters. Generally speaking, however, no two of these agree in what should be the minimum permissible tensile strength, or variation in diameter, and many of them include other requirements such as a minimum elastic limit, a minimum number of twists made in different ways and on different lengths of wire and at different rates of rotation. Most of the specifications require a certain minimum elongation, which is measured in various original lengths and under different conditions. It naturally is often the result that in a given specification an attempt has been made to cover all of the apparently desirable characteristics; and it sometimes happens that the maxima in opposed properties are stated as the specified requirements.

The electrical conductivity of the wire is probably the most nearly fixed of all the characteristics demanded; and yet there is an unexpectedly wide range in the electric conductivity called for in the existing specifications.

The tensile strength of hard copper wire is greatly influenced by the actual amount of the reduction in section of the wire in the cold-drawing operations, and nearly all of the specifications for

tensile strength are practical in that they can be met. Obviously, however, the elongation of the wire must decrease with increasing amounts of cold drawing; hence there must be a compromise between the maximum tensile strength and maximum elongation in order that hard wire may be made to meet specifications, and yet be produced economically. With respect to requirements such as the twisting and wrapping tests, it is a fact that the results vary so greatly with the way in which the test is made and with changes in outside influences, that unless all conditions are very carefully defined and observed, the results of the tests themselves are practically valueless.

Another matter which seems worthy of consideration is the variation in practice in specifying the gauge or size of wire, there being not less than three distinct wire gauges used, as well as some modifications or special interpretations of the values of the gauge numbers. It would greatly simplify matters and make reference definite, if some general agreement could be reached whereby the size would be specified in decimal parts of an inch without reference to arbitrary gauge numbers.

To indicate the large range of the requirements of existing specifications, we propose to give a few examples. One specification which recently came to our attention requires a tensile strength of not less than 60,000 lbs. per sq. in. with a minimum elongation of 1 per cent. in 10 ins. and an elastic limit of not less than 36,000 lbs. per sq. in., the specification appearing to apply to all sizes of hard-drawn wire which the company issuing it desired to purchase. The size of wire which was called for on a particular inquiry in the case just mentioned, is required in another specification to have a minimum tensile strength of 62,000 lbs. per sq. in. with a minimum elongation of 1.14 per cent. in 60 ins. Elongations are measured under this specification with stress applied to the wire, while it is to be presumed that under the first specification the elongations are to be measured after the wire has been broken. A third specification requires this same size of wire to have a standard strength of 64,200 lbs. per sq. in. without any elongation being specified. These three specifications may be considered typical. There is a difference of 7 per cent. in tensile strength actually required of a given diameter of wire, and, furthermore, a difference of 500 per cent. in the length upon which the elongation is to be

measured. One specification requires the wire to be broken and the elongation then to be measured between bench marks originally placed upon the wire, while another states the whole elongation which has taken place before fracture occurs, and includes the degree of elasticity remaining in the wire at the time of fracture.

There is another difference to be noted among the three specifications quoted. One of them apparently requires the same tensile strength in hard-drawn wire, regardless of its size; another demands that the strength increase uniformly as the diameter decreases. It is probable that the relation between diameter and tensile strength follows a curve rather than a straight line, and commercial practice in the manufacture of the wire is likely to cause jogs in this curve. If these jogs are not permitted in the curve, it will materially complicate manufacturing conditions.

The elastic limit of the wire is included as a requirement in some specifications and is omitted in others. That it is difficult to locate with any degree of exactness is obvious. The coiling of the wire sets it in a curve and leaves it in a state where one side is strained to a greater degree than the other when the wire is pulled straight in the testing machine. There is apparently no jog in the elastic curve corresponding to that at the yield point in steel; hence the elastic limit cannot be located, even with fair approximation, by the drop of the beam or increase in stretch rate in the manner customary in steel testing. Recourse must be had to the tedious method of taking extensometer readings, from which the elastic curve may be plotted. Because of the unevenly strained condition of the wire just mentioned, the elastic curve has puzzling inflections, probably caused by small sets taking place on the overstrained side. These make the location of the true elastic limit uncertain. The time consumed in making the test is more than a mill laboratory can usually afford. This, combined with the uncertainties in the results, makes it a debatable question whether it is not better to determine, with fair approximation, the probable ratio of the elastic limit to ultimate strength, for the benefit of the designer, but to omit the location of the elastic limit from specifications and as a regular inspection test.

A trolley line, or even a telephone line, does not present any difficult problems involving the actual physical strength of the wire of which the line is constructed, because the spans are short

by reason of the comparatively small sag that is permitted in such lines. With a long-distance transmission line the case is somewhat different. It frequently happens that the availability of a water power is to a considerable extent determined by the cost of the transmission line for conveying the power to the desired point of application. The cost of the transmission line is made up not only of the copper wire entering into it, but also of the poles, the insulators, and the means of suspension. The size and therefore the cost of the copper wire itself is to a large extent fixed by electrical considerations. Hence the variable factors in cost may be said to be suspensions, including poles, insulators, etc., and the longer the spans the less will this cost be. The permissible length of span is obviously determined by the physical characteristics of the wire, and the amount of sag which may be tolerated. Hence the engineer who is designing the transmission line must have accurate knowledge of the physical properties of the wire entering into it.

While hard-drawn copper wire and its uses may be of less general interest than some of the problems arising through the use of the more common engineering materials, the extension of electric driving in railroad practice brings this matter under the consideration of many more of our members than might at first be supposed. On this account, and with the hope of interesting the Society and having the subject taken up as has been done with many other materials, these notes have been prepared.

DISCUSSION.

Mr. G. E. Thackray.—It might interest the members who are familiar with current practice in measuring and stating diameters of copper wires to know that about ten years ago those interested in the manufacture of steel rods, hoops and bands specified that the gauging thereof should be done on the decimal system, which was advocated many years before that, namely, in 1877, by the American Institute of Mining Engineers, and later by the Amercan Society of Mechanical Engineers, The American Railway Master Mechanics Association and others. **Mr. Thackray.**

But only recently, during the present year, have those directly interested as manufacturers of the steel products above mentioned again determined to adopt and practice the decimal system of gauging. It seems to me that the decimal system simplifies the general problems and all matters connected with them and that it could be applied to the gauging of copper wire in connection with its manufacture and use, leaving calculations for conductivity to be made or expressed as heretofore in circular mils.

Mr. J. A. Capp.—On that question I might say that it has been the practice of two of the leading electrical manufacturing concerns for a great many years to endeavor to confine the specifying of sizes for wires, sheet metal and the like, entirely and exclusively to the decimal system, expressing dimensions in thousandths of an inch as the smallest point to which we can measure. The confusion from gauge numbers is not only exasperating, but infinitely worse than that. **Mr. Capp.**

Mr. W. H. Bassett.—Manufacturers of copper wire would be very glad if they could have the matter of decimal gauging bought up. Some of the large electrical concerns already specify in decimal parts of the inch, but most engineers in making specifications use gauge numbers. In some cases one gauge is used, and in some another. This makes it extremely perplexing for the manufacturer, and many mistakes have been made in interpreting gauge numbers. **Mr. Bassett.**

Mr. Skinner. MR. C. E. SKINNER.—I should like to add my quota to the discussion in regard to the desirability of using decimals in designating sizes of wire and sheet and other materials usually designated in the trade by gauge numbers. About four or five years ago we found so much confusion in our factory due to the use of the large number of gauge systems that we were compelled to adopt the decimal system. We found that we were using different diameters of wire of the same general class of material having identical gauge numbers. For example, when No. 18 was called for, nobody was quite certain—unless the diameter was also given—what size of wire was required. We therefore issued a small pamphlet to all our departments stating that after a certain date all papers, specifications, orders, etc., used in transacting our department business, must give the dimensions of wire, sheet, etc., in decimal sizes. The greatest opposition to this plan has come from the men designing electrical apparatus, and who are entirely familiar with the Brown and Sharpe gauge system. This system is very convenient on account of the fact that it is easy to remember that the area of cross-section of wire doubles with every three numbers. Therefore, when the resistance, cross section, weight, etc., of a few numbers are kept in mind, the other sizes can be very readily derived mentally. For all materials, such as sheet and wire, except copper magnet wire, the plan has worked out with entire satisfaction, and the opposition to the plan for copper wire is rapidly dying out. In the use of this system we have not changed the dimension of any material, but we merely designate the various materials in decimal sizes equivalent to the diameters of the various gauge numbers for that particular material as used regularly in the trade. It would be a very great benefit to us if such a system became universal.

THE STRUCTURAL MATERIALS TESTING LABORATORIES, UNITED STATES GEOLOGICAL SURVEY. PROGRESS DURING THE YEAR ENDING JUNE 30, 1908.

By Richard L. Humphrey.

The work in the Structural Materials Laboratories of the United States Geological Survey during the twelve months ending June 30, 1908, has involved the making of 20,000 tests. During this period there has been completed the greater part of the installation of equipment previously undertaken including the 600,000-lb. universal vertical screw testing machine, with a capacity for columns 30 ft. long, transverse specimens of 25-ft. span and tensile specimens 25 ft. long. The acceptance test on this machine proved it to be thoroughly up to the specifications. A structural steel column 30 ft. long was placed in the machine and loaded with 600,000 lbs. At this load the machine was sensitive to less than 200 lbs. The test piece was so placed in the machine as to bring the bottom 3 ins. off center in one direction and the top 3 ins. off center in the opposite direction. The full load was applied to the test piece and the guide columns showed a movement of $\frac{1}{4}$ in. at the top, the machine going back to its original position on the removal of the load. A bar of $4\frac{1}{2}$-in. cold-rolled shafting, 16 ft. long was placed in the machine and a tensile load of 600,000 lbs. placed upon it and repeated several times. At maximum load the machine was sensitive to less than 200 lbs. The bar was then nicked and broken at 550,000 lbs., throwing a full recoil of almost 600,000 lbs. upon the machine. The machine stood this test without moving from the knife edges; the recoil device worked perfectly. A beam was placed on a transverse arm and a load of 185,000 lbs. delivered on the cross arms.

In addition to this equipment there is being built for the laboratories a 10,000,000-lb. vertical compression machine capable of testing in compression a test piece 60 ft. long, with a platform

capable of accommodating specimens 6 x 6 ft. in section. The machine consists of a press operated by hydraulic power having sufficient cylinder capacity to develop a load of 10,000,000 lbs. in compression, the load being recorded by means of a system of scale levers, each division on the dial of the beam being equivalent to 100 lbs. load. Smaller readings are possible by means of an additional needle beam. The upper head is adjustable on four screws by means of gearing attached to the head.

Power is applied by means of a 15-horse-power 220-volt variable-speed motor operating a triple-plunger pump. The machine is to have an adjusting speed of 10 ins. per minute and a variation in the rate of applying load ranging from $\frac{1}{60}$ in. to $\frac{1}{2}$ in. per minute. It is expected that this machine will be delivered early in the coming fiscal year.

In describing the work which has been done it will perhaps be most convenient to take it up by sections.

The examination of the constituent materials of cement mortars and concretes from various parts of the country has involved the collection of samples from Wisconsin, Minnesota, Iowa, Indian Territory, Texas and Ohio. The samples consisted of sand, gravel, and broken stone, collected by geologists connected with the laboratory whose report contains a description of the deposit and the output and use to which the material is being put. Over 9,000 tests of various kinds have been made in this department. These materials have been tested, in addition to tests of their physical properties, as mortars made with typical Portland cement and as the aggregate of concrete using typical cement and standard Meramec River sand.

The investigations of the properties of plain and reinforced concrete beams, together with tests of the corresponding compression cylinders 8 ins. in diameter by 16 ins. long, are being continued. These tests include beams made of various proportions and aggregates, reinforced with different percentages of mild steel rods. For every aggregate and proportion of concrete, every age, and every diameter of rod, a bond test was made. In order to have a record of the quality of the steel used in these beams the yield point of every piece of steel was determined. This required making 450 tests for 1-in. round steel and 401 tests on $\frac{1}{2}$-in. round steel. The year has seen the completion of

the tests of 1,100 reinforced concrete beams, many of the results of which have already appeared in bulletins issued by the United States Geological Survey and others of which are being prepared for publication as rapidly as possible.

In order to ascertain the variation between the strength of beams made under the uniform conditions which prevail in the laboratories, and conditions which prevail in actual work, a series of tests was inaugurated in which three large concrete construction companies in St. Louis and the Structural Materials Laboratories tested beams, of which some were exposed to the outside weather conditions and others were stored in the laboratory. The results of these tests give an indication of the possible factors of safety which should be allowed in practical work. The series will be expanded to include another series of tests using a richer mixture. In the first series ninety beams in all were made and tested.

A preliminary series of tests of reinforced concrete slabs has been completed. Seven slabs in all were made and tested. These slabs were supported at two edges and also at four edges, and were reinforced with ½-in. rods running in both directions in the case of slabs supported at four edges, and in one direction in those supported at two edges. Expanded metal and welded wire cloth were also used in the tests. These metal fabrics were used running in one direction only and in both directions. The series was planned for the purpose of trying out the methods to be used in testing, in particular the method of using water in applying the loads. A wooden box with an interior waterproof canvas bag by which a uniform load could be applied rested on the slab and is believed to have given an almost ideal uniform load. The slabs were thicker than perhaps they should have been, being 3 ins. deep to the center of the steel, with 1 in. covering underneath.

In the Permeability Section, the work of the previous year of investigating the effect of uniform grading and wet mixtures on permeability, has been extended, and some forty different types of waterproofing compounds, covering powders, liquids, and surface coatings, have been tested under 20 lbs. water pressure with a view of determining whether they had any merit either as waterproofing or dampproofing compounds. Complete chemical

analyses were made of every compound and the results will be published in the early part of the fiscal year in bulletin form.

In the Cement Hollow Block Section, the work of last year has been largely completed and the physical properties have been determined on about 2,000 blocks. In addition, a number of series of fire tests have been made in the Underwriters' Laboratories in Chicago, which cover thirty panels and include tests of limestone, gravel, granite, and cinder concrete, and also of brick terra cotta, tile, limestone, marble, sandstone and granite, besides several varieties of red brick.

The laboratory has also been making a number of tests of wire rope of 1¼ ins. and 1½ ins. diameter, the ultimate strengths of which were 120,000 lbs. for the 1¼-in. rope and 175,000 lbs. for the 1½-in. rope. This involved also the making of 300 tension tests and the same number of torsion tests on the wire. These cables were tested for the Isthmian Canal Commission.

About 2,300 samples have been submitted for chemical analysis during the year. These include a number of samples of concrete material for the Reclamation Service, which is examined particularly for the amount of alkalis present in order to throw some light, if possible, upon disintegration which has been reported on several projects.

An examination was also made of copper wire screenings from various points along the Isthmian Canal, in the endeavor to discover the cause for the rapid corrosion of wire screening under atmospheric conditions in the Canal Zone. Chemical analyses were also made of the wire used in wire rope, and of the various waterproofing compounds, besides a routine analysis of cement, sand, stone, gravel, clay and silt.

In addition to this, special investigations have been undertaken concerning the effect of salt water and alkalies on concrete; also investigations of the fire-resisting properties of various building materials.

The work during the coming year will involve an extension of these tests and also a series of tests on columns, various building stones, and the extension on a much larger scale of the investigations of clay products. The laboratory has now been gotten into an efficient working condition under a thorough organization, and equipped with all the appliances necessary to

make the various tests. The results of the work thus far have been published in a number of Government bulletins, the organization, equipment, and methods being described in Bulletin 329, while the results appear in Bulletins 324, 331, 344 and others, all of which may be obtained upon application to the Director of the United States Geological Survey.

THE INFLUENCE OF FINE GRINDING ON THE PHYSICAL PROPERTIES OF PORTLAND CEMENT.

By Richard K. Meade.

The effect of the fineness to which Portland cement clinker is ground upon the physical properties of the resulting cement is well understood and it is not the purpose of this paper to deal in generalities along this line but rather to present to the members of the Society the results of some carefully made experiments to determine the actual commercial value of fine grinding. The two properties of cement most affected by the fineness of the product are the setting time and the sand carrying capacity. All the properties of the cement are of course influenced to some degree. For instance, the color of clinker itself is practically black. As the clinker is ground the color becomes lighter, until at a fineness of 75 per cent. passing the No. 200 test sieve, the color of the commercial product, a light buff, is reached. Cement ground so fine that 95 or 100 per cent. of it will pass the No. 200 test sieve is of a somewhat lighter shade than cement ground to the ordinary fineness of 75 per cent. passing this sieve. At the same time, no manufacturer would care to go to the increased expense of grinding the cement to such an extreme degree of fineness merely for the sake of a slightly lighter color, nor would even sidewalk and concrete-block men care to pay the increased cost of such cement simply to obtain a slightly lighter shade in their products.

Fine grinding will also to some extent help the soundness of the cement. This is shown by Table I. No manufacturer, however, would expend his energies on the grinding of the hard clinker to an impalpable powder in order to secure soundness, when half this amount of energy expended upon the grinding of the raw materials would be much more sure of producing the same results.

Table I gives four instances in which soundness was helped by fine grinding, but in order to obtain these four instances many samples of unsound cement were ground, and the majority of them failed to become sound even after being ground to an impalpable

powder. Fine grinding and seasoning, however, usually produced the desired results. That is, an unground cement after seasoning, say one week, failed to pass the boiling test, but the same cement ground so fine that none of it remained on the No. 200 test sieve passed the test after seasoning one week. The grinding no doubt here breaks up the small pieces of clinker and allows the air to slake out the injurious component. In this connection, it may be said, that if the coarse particles, i. e. those remaining on the No. 200 sieve, are separated from the cement and ground to a fineness of 75 per cent. through the No. 200 sieve, the resulting product is usually unsound. It is also usually quick setting, due to the fact that the sulphate nearly all passes into the fine powder. If 1 per cent. of plaster of Paris is added to the powder, its setting time is

TABLE I.—SHOWING EFFECT OF FINE GRINDING OF CEMENT ON SOUNDNESS.

Cement Number.	Result of Five-Hour Steam Test. (A. S. C. E.)		
	As received.	Ground to pass No. 200 sieve.	Ground to an impalpable powder.
1	Checked.	Sound.	......
2	Checked.	Sound.	......
3	Checked.	Slightly checked.	Sound.
4	Checked.	Slightly checked.	Sound.

normal but it is still unsound. If the powder is then seasoned for a few days it becomes sound.

As we have said before, no manufacturer could afford to make a sound cement by grinding one unsound at ordinary fineness to say 100 per cent. passing a No. 200 test sieve, as his competitors by grinding the much softer raw materials to a fineness of only 95 to 98 per cent. through the 100-mesh sieve would be practically sure of obtaining the same results, provided the composition of the mixture and the burning of the clinker were satisfactory. At the same time, if the manufacturer found it advantageous to grind his cement to a fineness of 90 to 95 per cent. through a No. 200 test sieve, he would find that it had some beneficial effect upon the soundness also, and that this effect was most marked where the cement had a chance to season or age as it usually does.

The influence of fineness upon the rate of set of cement is in some instances quite marked; in other instances this is much less noticeable. If any effect is produced at all, and there generally is, it is to make the cement quicker setting,—in some instances, so quick setting as to be unfit for use; and often, where this is the case, additions of plaster of Paris fail to retard the set sufficiently to allow the cement to be used. In Table II are given a number of instances illustrating the influence of fine grinding upon setting time.

TABLE II.—INFLUENCE OF FINE GRINDING OF CEMENT UPON ITS SETTING TIME.

Setting Time (Initial Set) in Minutes.

Cement Number.	Per Cent. Passing a No. 200 Sieve.					
	75%	80%	85%	90%	95%	100%
1	255	246	192	75	12	2
2	105	106	100	100	22	6
3	120	115	100	95	60	35
4	240	200	180	115	60	30
5	240	210	110	55	15	5
6	200	190	175	100	25	2
7	100	100	90	80	25	5
8	115	105	100	75	30	10

The question of the influence of fine grinding upon the set is an important one, for upon this will depend to a large degree the ability to grind cement to the point where all of it is rendered useful, and where it contains no inert matter except that present chemically and not due to coarseness. The composition of the cement unquestionably has something to do with the effect of fine grinding. High-alumina and low-lime cements seem to have their setting time most affected by finer grinding. High-lime, soft-burned and low-alumina cements do not seem to be so much affected.

Cements low in lime are often quick setting, and if a sample of cement is sieved through a No. 200 test sieve and analyses are made of both the coarse residue and the fine portion passing, the former will in most cases be found lower in lime than the latter. It is natural that the softer portions of the clinker should constitute the greater part of the impalpable powder in ordinary Portland cement. When the cement is ground still finer the harder portions also are

broken up, and these harder portions are probably responsible for the "quick set" of finely ground cement, owing to the fact that they are lower in lime and are burned to a high degree of vitrifaction. It is certainly possible, even probable, that if it is found advantageous to grind cement to a much greater degree of fineness than is now practiced, it will also be found necessary to grind the raw materials to a high degree of fineness, in order to allow the making of very highly basic cement, in which the highest possible amount of lime is obtained in solution in the magna of clinker. If it is desirable to get rid of all the physically inert material by fine grinding of the clinker, it is also equally desirable to have in the cement all of the chemically active element possible.

I am strongly inclined to believe that it will be possible to grind cement very fine without influencing the set unfavorably, by properly adjusting the composition of the clinker and the degree of burning. If the finer particles of cement, not merely the particles which pass a No. 200 sieve but the impalpable dust, are separated from the cement, it will usually be found that this very fine material sets normally, showing that it is possible to grind some part of the cement at least to an impalpable powder. It is also now generally agreed that it is this fine powder which is the active constituent in cement. Hence it follows that the active portion of cement is not quick-setting even when finely ground, and that there is some undesirable element in the coarser and at present inert particles of the cement which is liberated or rendered active by the grinding. The problem will therefore undoubtedly be to keep out the undesirable element from the clinker and to increase the desirable one. I have no doubt that by the time grinding machinery has been perfected which will reduce cement to the fineness of 100 per cent. through a No. 200 test sieve on a commercial basis, the chemical side of the question will also have been solved. Indeed, experiments made by the writer indicated a solution of the problem. Under present conditions it would be practically impossible to produce commercially a cement much finer than 90 per cent. passing a No. 200 sieve, if indeed it would be possible to reach even this fineness, and at this nothing more than a slight shortening of the setting time of properly proportioned cements should be met with.

A number of experiments were made by the writer to deter-

mine the effect of finer grinding upon the tensile strength of Portland cement. All of these experiments proved the following general facts.

1. That the NEAT strength is *lowered* by finer grinding.

2. That the SAND strength is *increased* by finer grinding.

These two facts are shown by every series of tests made by the writer. Table III gives the results obtained in one of the most carefully made of these tests. Each figure in this table is the average of five closely agreeing breaks and all briquettes and breaks were made by an experienced tester. All five samples are from *the same* lot of cement. In fact, a bag of cement was selected and divided into five parts and each of these ground to a different degree of fineness as shown below:

Fineness passing No. 200 sieve.	Fineness passing No. 100 sieve.
80%	93.9%
85%	95.8%
90%	97.4%
95%	99.0%
00%	100.0%

Referring to Table III we see that the neat strength is decreased by fine grinding. This decrease is as follows: Grinding to 85 per cent. fine decreases the 7-day neat tensile strength 17 per cent. and the 28-day neat tensile strength 11 per cent. from the figures of the 80 per cent. fine. Grinding to 90 per cent.

TABLE III.—STRENGTH OF THE SAME CEMENT GROUND TO VARIOUS DEGREES OF FINENESS.

Tensile Strength in Lbs. per Sq. In.

Age in Days.	Neat or Sand.	Per Cent. Passing a No. 200 Sieve.				
		80%	85%	90%	95%	100%
1	Neat.	369	241	308	282	200
7	Neat.	955	796	749	627	558
28	Neat.	963	849	775	626	594
7	1 : 3 Sand.	235	284	351	363	382
28	1 : 3 Sand.	297	353	468	498	576
7	1 : 4 Sand.	160	204	234	247	263
28	1 : 4 Sand.	224	266	324	377	392

NOTE.—Each value is based on five briquettes.

fine decreases the strength 21 and 20 per cent. respectively for the same periods. Grinding to 95 per cent. fine decreases the strength 34 and 35 per cent. and grinding to 100 per cent. fine decreases it 42 and 38 per cent. In general it will be seen that the decrease in neat strength due to fine grinding is about the same for both the 7-day and the 28-day periods.

Referring to the sand tests it will be seen at a glance that the increase in sand strength due to finer grinding is large. Increasing the fineness from 80 to 85 per cent., increases the 7-day 1 : 3 sand strength 21 per cent.; further grinding to 90 per cent. increases it to 45 per cent.; grinding to 95 per cent. increases it to 54 per cent., while grinding to 100 per cent. increases it to 63 per cent. over the 80 per cent. strength. The 1 : 4 sand strength is increased by practically the same percentage. The increase upon the 28-day sand tests due to finer grinding is even larger. In this series of tests the original cement gained but little neat strength between the 7 and 28-day periods. In the case of cements showing good gain between these two periods, fine grinding will decrease not only the neat strength but also the percentage of gain between these two periods as well. An example of this is given below. In this experiment a lot of cement, just as received from the mills, was divided into two parts, one of which was tested just as it was and the other was ground to completely pass a No. 200 sieve, and then tested. Below are the results obtained on the two samples:

TENSILE STRENGTH IN POUNDS PER SQUARE INCH.

Neat.

	1 day.	7 days.	28 days.	3 mos.	6 mos.	1 yr.	2 yrs.
As received......	327	630	725	730	700	825	850
Ground to pass a No. 200 sieve ..	210	525	540	540	560	575	560

1 : 3 *Mortar.*

	1 days.	28 days.	3 mos.	6 mos.	1 yr.	2 yrs.
As received......	278	357	387	390	410	425
Ground to pass a No. 200 sieve..	480	555	575	615	623	640

The question naturally arises as to the commercial value of fine grinding. In other words, "is the game worth the candle?" Will the benefit to be derived from a much finer product than is at present on the market be sufficient to warrant the increased cost of

its production? No very settled figures are obtainable as to the relative cost of grinding to different degrees of fineness. In an experiment made by the writer with a small tube mill in which the charge was put and allowed to remain until the desired fineness was obtained, the time required for grinding the material from the ball mill was as follows:

Per cent. passing a No. 200 sieve.	Hours required to grind to this fineness.
75	19
80	21
85	23¼
90	26

On the basis of this test, increasing the fineness from 75 to 90 per cent. through a No. 200 sieve would only increase the cost of grinding 37 per cent. As the cost of grinding the clinker is never more than 15 per cent. to 20 per cent. of the cost of making a barrel of cement, the increased cost of grinding 90 per cent. through a No. 200 sieve over 75 per cent. would be only from 5.5 per cent. to 7.4 per cent. of the mill cost, and of course much less than this percentage of the cost of cement delivered on the work.

Referring to Table III, we see that cement ground to a fineness of 90 per cent. passing a No. 200 sieve, when mixed with four parts of sand, gives mortar of about the same strength as that obtained from ordinary cement (80 per cent.) mixed with three parts sand. Hence only 75 per cent. as much of the fine cement is needed as is required of the ordinary cement. Hence we reduce the amount of cement 25 per cent. and only increase its cost say 6 per cent. Or at a mill cost of $1.00 per barrel for ordinary cement, the finer product would be increased to $1.06 and the cost delivered increased, say from $1.50 to $1.56. One hundred barrels of ordinary cement would then cost $150, and 75 barrels of the finer product, which would do the same work, would cost only $116.

At this rate it is not hard to make out a strong commercial argument in favor of fine grinding. I am not sure, however, that the present style continuous feed and discharge tube mills are capable of reducing clinker to such extreme fineness as 90 per cent. through a No. 200 sieve, with an expenditure of only 25 per cent. additional energy. Tests seem to indicate that much more than

this is actually required and that the output of the mill would be at least cut in half. Even at this figure the fine cement would be of commercial advantage, for the fine cement would increase the cost of grinding the clinker 100 per cent., or would make the mill cost of fine cement say $1.20 against $1.00 for the ordinary, and the cost delivered say $1.70 against $1.50. One hundred barrels of ordinary cement would then cost $150, and 75 barrels of the very fine cement $127.50.

In this connection some figures obtained with the Fuller-Lehigh mill are interesting as actual results with a commercial mill on clinker. In tests, this mill pulverized clinker that had passed through $\frac{1}{4}$-in. mesh at the rate of from 10 to 12 barrels an hour to a fineness of 90 per cent. through a No. 200 sieve, utilizing in so doing 75 H.P. hours. A test of the product of this mill ground to 90 per cent. gave the following tensile strength:

TENSILE STRENGTH OF MORTARS IN POUNDS PER SQUARE INCH.

Age.	1:3 Mortar.	1:4 Mortar.
7 days	320	228
28 days	455	316

It was furthermore found possible by closing the lower part of the screens of this mill to grind cement so fine that all of it passed the No. 200 sieve. A test of this cement, the finest ground by a commercial mill that the writer has ever been able to obtain, gave the following figures.

TENSILE STRENGTH OF MORTARS IN POUNDS PER SQUARE INCH.

Age.	1:3 Mortar.	1:4 Mortar
7 days	382	291
28 days	438	348

It will be seen that the 7-day figures agree very closely with those obtained on cement all passing a No. 200 sieve in the experiment given in Table III. The failure to show the same strength in 28 days was no doubt due mainly to the fact that the cement was tested fresh from the mill.

Before leaving the subject of fine grinding I can not resist the temptation to again call attention to facts which I first brought forth about five years ago, namely, that fine grinding without an increase in the amount of impalpable powder gains nothing. It would be easily possible to prepare a cement of which 90 per

cent. or even 100 per cent. would pass a No. 200 test sieve and yet which would in reality be of much less value than the coarsest commercial product now on the market. For example, if a sample of clinker is crushed in an iron mortar by a pestle and sieved as fast as it is ground through a 100-mesh screen, a product will be obtained 100 per cent. of which will pass a 100-mesh screen. If a pat is made of this cement it will just about cohere. To go even further, if another sample is ground in a mortar and sieved after every few strokes of the pestle through a 200-mesh screen, the resulting product will all pass a 200-mesh screen and yet it will be almost worthless as a cement. When washed free from its flour with benzine it will just about hold together. In the writer's laboratory there is a Braun's Gyratory muller for grinding samples, in which the grinding is done by an enclosed round pestle revolving in a semi-hemispherical mortar. In the bottom of the mortar is a hole which can be stopped by a plug. The grinding may be done in two ways: one by feeding the sample into the hopper in the cover and allowing it to work its way out at the bottom, then sieving out the fine material from the coarse, and returning the latter through the grinder and so on until all has passed the sieve; the other, by placing the plug in the bottom of the mortar and allowing the pestle to work upon the material until the latter has reached the desired fineness. Two samples of cement were prepared from the same lot of clinker by these two methods. One sample, the one made by passing the clinker through the muller and sieving out the 200-mesh particles after each grind, would of course all pass a 200-mesh sieve. The other sample, the one made by grinding the whole sample to the desired fineness without screening, tested 96 per cent. through a 100-mesh sieve and 76.5 per cent. through a 200-mesh sieve. Sand briquettes were made of these two lots of cement with the following results;

TENSILE TESTS OF MORTARS SHOWING THE EFFECT OF SCREENING DURING THE GRINDING.

	7 days.	28 days.	3 mos.	6 mos.
Samples made by grinding and screening through a No. 200 sieve.	Broke in clips.	Broke in clips.	Broke in clips.	28 lbs.
Grinding to fineness without screening	215	295	325	318 lbs.

The cementing value of Portland cement depends upon the percentage of those infinitesimal particles which we call flour.

No sieve is fine enough to tell the quantity of these present. At the same mill it is probable that the sieve test is relative, but to the engineer who is called upon to examine the product of many mills using different systems of grinding the sieve test is hardly to be expected to give the relative percentage of flour in each. The products of the Fuller-Lehigh, the Griffin mill and of the ball and tube mill differ much in the percentage of flour present, although testing the same degree of fineness on the 200-mesh sieve. Even with the ball and tube mill system, one ball mill and two tube mills would probably give a product with a higher percentage of flour than one tube mill and two ball mills, although the cement was ground to the same sieve test. The size screen on the ball mills probably also influences the percentage of flour in a product of a certain fineness.

HYDRATED LIME AND CEMENT MORTARS.

By E. W. Lazell.

It has long been recognized as advantageous to increase the strength of lime mortars by the addition of cement; especially is this true when lime mortar is used where quick strength is desired, or where there is a large percentage of openings in the wall. Natural cement, or Portland cement, mixed with lime paste, has sometimes been used for this purpose.

Some investigations made a few years ago with lime paste show the advantage of the addition of cement. It was, however, difficult to obtain an even and uniform mixture of the three ingredients, lime paste, cement, and sand. The difficulty of this will at once be apparent when we remember that the lime paste is a wet tenacious material, difficult to mix with the drier ingredients. Further, unless the lime paste has been carefully prepared and allowed to age, there is almost certain to be unhydrated lime present which will later hydrate and expand, thus injuring the strength of the mortar.

Within the last few years there have been placed on the market prepared hydrated limes. These are nothing more than lime slaked with sufficient water, so that each particle of quicklime or calcium oxide receives enough water to form the hydrate. It is chemically the same material as lime paste without the excess water to make it wet and plastic. To render the hydrated lime plastic it is only necessary to add this excess water, in the same way as it is added to cement. As the hydrate is prepared in a regular plant designed for the purpose, with special machinery, and under the supervision of competent men, a much more uniform product is produced than by the customary way of slaking lime on the work. The mechanically treated hydrated lime is further aged in bins or silos, like cement, before it is placed on the market.

There are some engineers and architects who consider hydrated lime to be simply air-slaked lime. This is not true; the two materials are radically different in their composition and

TABLE I.—TENSILE STRENGTH OF MORTARS STORED IN AIR.
Lbs. per sq. in.

1:3 Mortars.

Per cent. of Cement replaced by Limoid.	0	5	10	15	20	25	30	100
Lab. No.	39950	40010	40020	40130	40190a	41560	41680	40250
7 days	209	203	205	209	133	112	141	10
28 days	266	258	255	245	203	170	225	44
3 months	286	289	295	297	197	117	177	55
6 months	382	312	304	281	229	211	219	53
9 months	607	545	441	499	441	397	342	136
12 months	630	456	513	642	553	444	327	168

1:5 Mortars.

Per cent. of Cement replaced by Limoid.	0	5	10	15	20	25	30	35	40	100
Lab. No.	40520	40580	40640	40700	40760	40820	40880	41800	41860	40940
7 days	96	53	78	63	72	75	71	104	105	0
28 days	79	49	67	77	96	190	112	186	164	18
3 months	90	65	108	88	115	151	145	154	149	27
6 months	120	57	67	74	148	215	152	190	176	30
9 months	277	247	324	346	372	351	346	336	285	108
12 months	311	238	334	331	389	348	346	349	295	151

NOTE.—Each value is the average of five briquettes.

characteristics. Air-slaked lime has become slaked by contact with the moisture and carbon di-oxide of the atmosphere; it is not of a uniform composition, and generally contains considerable free or unslaked lime. On the other hand, hydrated lime is a homogeneous uniform product, perfectly hydrated.

This product comes into the market as a dry powder in bags, and can be as easily handled as cement; therefore, for the preparation of lime mortar or lime cement mortar it offers the great advantages of ease of handling and uniformity.

Cement mortars are short, that is, they do not spread easily under the trowel, but they have great strength and develop this strength quickly. Hence the addition of a material to make the mortar work smoothly is readily recognized as advantageous. It was with a view of learning the amount of hydrated lime that could be advantageously added to cement mortars and the strength of the various mixtures that the following investigations were undertaken. As hydrated lime is considerably more bulky than cement, it materially increases the volume of the mortar.

In these experiments, Nazareth Portland cement and Limoid

(a hydrated dolomitic lime prepared by the Charles Warner Company) were used. The sand was standard Ottawa sand and of such size that all passed a No. 20 sieve and was retained on a No. 30 sieve. This sand was selected so that the results might be compared with those obtained in testing cement.

In the tests sets of five briquettes were used, and all test pieces were stored in the air of the laboratory. Once a week the briquettes were moistened with water. Table I gives the results obtained.

The results clearly show that the replacement of 15 per cent. of the cement by hydrated lime in both the 1:3 and 1:5 mixture did not decrease the strength; and that when 25 per cent. and 30 per cent. of the cement was replaced by hydrated lime the resulting mortars were amply strong. It should therefore be possible to use in mortars for brick work an amount of hydrated lime equal to 25 per cent. of the cement used, obtaining thereby a plastic mortar which is much stronger than lime mortar, and gains its strength much more quickly. In cement mortar the addition does not materially decrease the strength and it does, to a marked degree, increase the plasticity. The mortar would therefore be easier to place and laying the bricks would be much facilitated. The plasticity of the mortar would also enable the bricklayer to do better work.

The results of these tests led to the investigation of the effect of hydrated lime on mortar exposed to the action of water. Similar 1:3 mixtures were prepared, the briquettes stood three days in the air and were then placed under water in tanks like cement briquettes. Table II gives the results obtained.

TABLE II.—TENSILE STRENGTH OF 1:3 MORTARS STORED IN WATER.

Lbs. per sq. in.

Per cent. of Cement replaced by Limoid.	0	5	10	15	20	25	30
Lab. No.	39890	40310	40370	40430	40490	41620	41740
7 days	206	157	189	239	237	173	173
28 days	278	311	364	264	268	259	268
3 months	441	389	419	372	374	314	281
6 months	358	321	341	278	260	207	253
9 months	390	301	308	279	268	250	232
12 months	426	336	311	322	299	260	231

NOTE.—Each value is the average of five briquettes.

These results indicate that quite large amounts of hydrated lime can be added to cement mortars even when they are exposed to the action of water. The hydrated lime in this instance acts as a filling material and since it is of a collodial nature it should tend to render the mortar more impervious to water.

In order to investigate the permeability of such mortars, circular pats were made of the different mixtures, 3 ins. in diameter and 1 in. thick. These were then placed in an apparatus such that water under pressure could act upon them in the center through an opening exactly 2 ins. in diameter. Thus the area acted upon was 3.1416 sq. ins. All pats were subjected to a pressure of 30 lbs. per sq. in. for one hour. All the pats for the twenty-eight-day test were kept submerged in water until the time of testing. The pats for the six-week tests were taken from the tanks two weeks before the test and thoroughly dried out; in this condition they were subjected to the test. The results of these tests are given in Table III.

TABLE III.—PERMEABILITY TESTS OF MORTARS.

Lab. No.	Proportions of Mortar.	Per cent. of Cement replaced by Limoid.	Age.	Amount of Water, at 30 lbs. per sq. in. Pressure, Passing Through Test Piece in 1 Hour. c.c.
41050	1:3	0	7 days	10
41060	"	5	"	5
41070	"	10	"	2
41080	"	15	"	0.5
41055	"	0	28 days	0
41065	"	5	"	0
41075	"	10	"	0
41085	"	15	"	0
41095	"	20	"	0
43540	1:5	0	"	3000
43550	"	5	"	5
43560	"	10	"	2.5
43570	"	15	"	0
43540	"	0	6 weeks	1090
43550	"	5	"	3
43460	"	10	"	0
43570	"	15	"	0
43580	"	20	"	0

NOTE.—Each value is the average of two tests.

Referring to this table it will be seen that the additions of even small amounts of hydrated lime materially decrease the permeability of the mortar.

These tests, then, seem to indicate that in hydrated lime we have a suitable material to add to cement mortars to render them plastic, thus increasing the sand carrying capacity and the ease of working without materially reducing the strength; 1:5 mixtures containing an amount of hydrated lime equivalent to 35 per cent. of the cement used are amply strong for all purposes, and are practically impermeable to water. These mortars should also increase in strength if exposed to air, through the absorption of carbon di-oxide, as is the case with lime mortars.

It therefore seems to be advisable for architects, engineers and builders to give this material careful consideration in view of its many desirable characteristics.

STANDARDS FOR PORTLAND CEMENT, ESPECIALLY FOR THE TENSILE STRENGTH.

By W. W. Maclay.

This paper is a plea for conservatism in our standards for Portland cement, and although the ground gone over must be very familiar to many, its repetition may be found useful as tending to diminish the friction between the manufacturer and the consumer, while at the same time preserving the standard of real excellence in the cement.

The manufacture and physical testing of Portland cement are at the best only approximations, especially as compared with the determination of its chemical constituents. It seems unreasonable, therefore, for the consumer to exact in the physical tests for tensile strength any but the most liberal standards, and those which can always be attained with average certainty by the manufacturer.

The writer has made the tests and standards for tensile strength the principal subject of this paper, because the tensile strength test of Portland cement is easily made, and is perhaps one of its most characteristics tests. It will therefore probably always remain a favorite and interesting theme in cement literature and competition.

Although the Standard Specifications for Cement adopted by the Society have been in very successful operation for several years, many engineers still cling to their own opinions as to standards. And generally wishing to raise this Society's standards for tensile strength, they prescribe a percentage increase between the 7- and 28-day periods, in addition to the present average minimum requirements.

Testing under the present standard specifications for strength has generally been found satisfactory to both sides, because the minimum standards have purposely been placed low, and yet high enough to insure a good quality of Portland cement, and one that is amply strong for any engineering construction. Reference is now made to the neat test at the 7- and 28-day periods; the one day standard is so low, and in some ways unimportant, that it can

be overlooked in this discussion, as well as the 7- and 28-day sand tests, which are simply derivations of the neat test.

The present standards are easily reached by good cement. And, while they could be raised some 100 lbs. each, without causing many rejections, the gain to the consumer would hardly be sufficient to justify the change.

As the minimum standards, averaging 500 lbs. in 7 days, and 600 lbs. in 28 days, insure getting a cement somewhat stronger, let us see what would be the result of increasing these standards 100 lbs. As the cement supplied under the increased standards would always be used with sand, raising the quality of the neat cement 100 lbs. would perhaps raise the quality of the three-part sand mortar derived from it some 30 lbs.; and because in building construction the safe unit strength for mortars, both in tension and compression, is obtained by dividing the unit breaking strength by 10, we would get by the above increase in standards only 3 lbs. additional in the safe working strength per square inch for our cement mortars, an amount of course that is entirely negligible in construction calculations. In this connection, a glance at the safe unit-stresses of the principal materials used in the present-day building construction will perhaps illustrate the point, of the small part played by the tensile strength of Portland cement in construction, although such a large quantity is used. The figures are as follows:

Portland cement, safe tensile strength per sq. in. ..	50 lbs.
Yellow pine, " " " " " ..	900 "
Steel " " " " " ..	18,000 "

In reinforced concrete for girders it is customary to ignore the strength of the concrete in tension, and to depend entirely on the steel reinforcement. Although in a general way it has been said that you cannot have cement too strong, yet if in building construction we do not make use of the excess strength that we now find supplied above the average of our Society's standards, what gain would there be in raising these standards?

Perhaps some one will say, that raising the strength (although it is not certain that by raising the standards we will raise the average market strength, as it is now much above the requirements) means that we can use more sand in our mortars; but this is doubt-

ful unless hydrated lime is mixed with the cement mortar to prevent it from being incoherent, and this practice is not popular in the United States, although common in Europe. Raising the present standard for tensile strength, therefore, seems unadvisable. The tensile strength tests made in a crude way at early periods were among the first used to show the remarkable difference between Portland cement and all other cements. And gradually came the feeling that the stronger the cement the better it was, until this was found to lead to over-limed, unsound cements; then opinion inclined to the other side, and early low strength with a definite increase between the 7- and 28-day period was advocated, which was all right with the old methods of burning. Since, with the rotary kilns now turning out uniformly a sound higher-limed product, the strength at 7 days is so much greater, and the increase between 7 and 28 days so much smaller than formerly, it is proposed to have a sliding scale in fixing the rate of increase, and to make the requirements less for stronger cement. But it seems to the writer that to require that the tensile strength shall increase within certain percentages between periods is asking the manufacturer to make a product for which there is no receipt. Le Chatelier, after fully investigating the early strength and rate of increase in Portland cement, practically gave up the subject as hopeless, and said that there is no law governing the growth of cement and thought that the mechanical tests in favor were "scarcely more instructive than the chemical analysis." This seems to be too pessimistic, for we have certainly found both the mechanical tests and the chemical analysis very useful, only neither of them should be made the subject of great refinement in our specifications or standards. If it were possible to make cement that would increase within definite limits, it would mean the control of the raw materials and of the burning beyond what we know to be practicable. Specifying these percentages of increase, therefore, would not give us better cement, because the manufacturer, unable to change his methods to comply with the specification, would simply do nothing.

It is also still a question, if the increase in tensile strength between 7 and 28 days is as good an indication of the ultimate strength of Portland cement, as the strength itself at the 7- or 28-day periods, considered alone. We must remember that rotary kiln burning has changed the character of our Portland cement so much

that its ultimate strength is now often shown at early periods, sometimes in 7 days and sometimes in 28 days, and as long as the cement is sound and normal this early strength should not be considered harmful. Cement burned in the old dome and ring kilns seldom if ever showed this great strength at short periods without being abnormal and overlimed. Hence it was then proscribed by engineers and the prejudice against it still exists.

Opinions once formed about a material possessing the subtle and elusive qualities of Portland cement die hard, even when proved again and again to be erroneous. It took over fifty years following the discovery of Portland cement by Smeaton to refute the error of a Swedish chemist that assigned the hydraulic properties of certain limestones to the manganese instead of to the clay which they contained, although most of hydraulic limestones in Europe were analyzed unsuccessfully to find manganese in support of the theory; and although they did find clay, the authorities persisted in contradicting Smeaton's clay theory almost up to Vicat's time. Many of the opinions and tests that surround the theory that the strength of cement increases between 7 and 28 days at a rate sufficiently definite to be made the subject of specifications and standards may some day share the fate of the long-buried manganese theory.

In the early days of English Portland cement making the cement was made from raw materials, poorly mixed, poorly burned and coarsely ground, and the manufacturers paid no attention to complaints because, although their product was poor, they could hardly supply the demand. It was then necessary for some engineer like John Grant, of the Metropolitan Board of Works, of London, to take hold of the situation, and by raising standards and rigid testing to greatly improve the quality of the commercial Portland.

To-day in the United States on account of the excellent character of the Portland cement factories and their output, no great change or improvement can be accomplished by engineers raising standards. The numerous factories are now able to supply more than the demand. The methods and machinery employed have been greatly improved and are much the same in all the mills, and the rotary kilns give a more uniform burning to a higher-limed product. The raw materials used in making the cement cover

vast areas and differ greatly, but since the resulting product, as regards its constituents, must come within such narrow limits to be a normal Portland cement, the chemical analyses of the various brands closely resemble each other. Even though they contain such constituents as magnesia and sulphur, slightly in excess of the present standards, their harmful effects have been altogether exaggerated. Besides being similar in the chemical analysis, the different brands tend also to approach each other more and more in their tensile strength, so that it would seem as if we were nearing the time when the present multiple standards for strength could be reduced and simplified with great advantage. We have now four standards for tensile strength (omitting the one day period), viz., the 7- and 28-day tests, neat and with sand.

Many tests have been made to show that the sand tests do not follow the neat tests absolutely, even when the cement is of the same fineness; many compression tests have been made to show that the compressive and tensile strengths of cement do not vary always in the same ratio. There are numerous advocates of the sand test alone, as well as of compression tests alone; yet it seems to the writer that the neat tests for the tensile strength are the simplest, most characteristic and uniform, considering our methods of burning and grinding. The German principle of having one period for testing, and only one kind of standard test for tensile strength seems deserving of our best consideration.

Of the two periods, 7 days seems to offer most advantages. As stated above, the tendency of our present day cement is more and more to show little gain between 7 and 28 days. Therefore, there is not much obtained by waiting for the longer period, and much to lose by having two periods or standards.

Much trouble and some expense has been experienced in getting a satisfactory standard sand. Eliminating the sand test as a standard and using it only for purposes of comparison would afford relief at once because of the small quantity of sand required. The sand test is a more difficult one to free from the personal equation than the neat test, and therefore many erroneous deductions have probably been made from the sand test. If authoritative tests, made according to the standard methods of the Society, were carried on for several years the ultimate strength of neat cement and

its sand derivatives would probably be found to vary in some ratio more uniform than is generally considered possible.

The writer does not expect that his views in favor of a single standard for strength will be immediately popular, owing to the long existence of the present system, and the many theories and beliefs that have grown up around it. But when the cement world gets tired of all the trouble that we now have in standardizing a sand for the whole country and in trying to reconcile the variations in a material like cement, that rarely varies the same way twice, a change will be in order.

Let us, therefore, continue making the 1-day, 7-day, 28-day and various long-time tests for tensile strength as formerly. Let us also make the allied tests for compression, adhesion and abrasion, etc., in order to establish reliable data for the extension of our scientific knowledge of cement, and also in order to study the differences and merits of the best brands. But for accepting cement on delivery, let us have some short, sharp and characteristic test for strength like 7-day neat test.

The long-time tests are very difficult to keep uniform and troublesome to make. It would be of great value if a series of authoritative tests, based on the methods of this Society, were started and carefully carried on through a period of ten years. In such a system of tests, the real retrogression at certain times would be noted and the limits of the changes more accurately defined than now. What we call retrogression would in many instances be found due to the personal equation, or to too much plaster, or to unsoundness in the cement. Let those who desire to increase present standards consider more carefully how this increase will help the work in which the cement is to be used, and if only a slight gain is indicated, it would seem better to postpone the changes.

We are perhaps apt to forget the essential differences between improving the standards for steel and cement. Improvement in the standard specifications for steel rails means the annual saving of many lives by preventing railroad accidents. But with the standard specifications for cement we are now getting a sufficiently strong and safe building material if it is only properly applied.

SANDS: THEIR RELATION TO MORTAR AND CONCRETE.

By Henry S. Spackman and Robert W. Lesley.

The purpose of this paper is threefold: to emphasize the fact that the strength of cement mortars and concrete is dependent as much on the sand as on the cement; to show the necessity of careful and systematic inspection and testing of the sand as well as the cement on all important construction; and to suggest the desirability of standard specifications for and standard methods of testing sand similar to those now adopted for testing cement.

To those conversant with the abuse to which cement is sometimes subjected, the wonder is not that we occasionally have a failure of concrete or mortar to harden, but rather that these failures should be so infrequent. Based on a comparison by weight cement forms rarely more than one-fourth the defective material, and more frequently is less than a tenth, this does not take into consideration one of the most important elements in the final strength but one that is not capable of measurement, that is, the handling and mixing. That cement should withstand, as it does, the almost inconceivable abuses it sometimes receives from "the man with the hoe" is the strongest testimony possible of its good qualities and value as an engineering material.

We recognize that the hardening of cement mortars and concrete is one of the most obscure and complex of chemical reactions and one that to date has baffled all efforts at solution. We know that the setting and hardening of cement is affected, more or less, by the surrounding conditions and that the strength developed is dependent on these conditions and guard against them in our standard specifications for testing cement, which prescribe in the most minute detail the temperature of the air and of the water, per cent. of water, character of sand, manner and time of mixing and gauging, pressure to be applied, etc., yet we make little or no effort to secure these same conditions in the work.

Omitting from consideration the varying personal element embodied in the labor which cannot be tested, and considering only the positive elements entering into the concrete or mortar, that is, the sand, stone and water, do you commonly see in any specification, no matter how elaborate may be the requirements as to cement, any provision for tests of these other materials? Yet

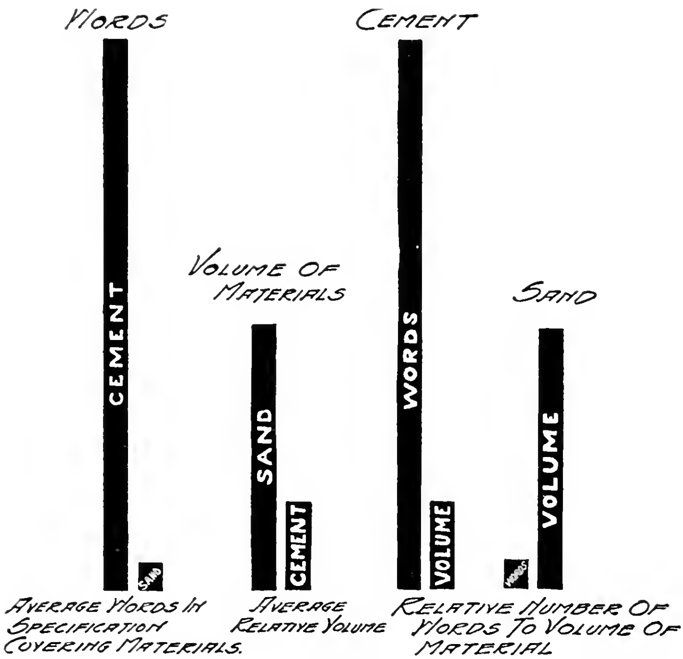

FIG. 1.—Graphic Representation of Relative Number of Words Devoted to Sand and Cement.
Average of Twenty-five Specifications.

they form by far the greatest portion of the mass and contribute, in proportion to their volume, to the strength of the resulting product and I think more frequently than any one realizes, to lack of strength when it develops. This is especially true of the sand. The average specification for sand is no different from that of the Romans or the builders of five hundred or a thousand years ago and contains little more than the vague requirement that it shall

be sharp and free from loam or dirt. There is no definite statement as to its chemical composition, graduation of size of grains, percentage of voids, or requirement as to strength it must give when mixed into mortar with a normal cement, all being left vaguely to the engineer or contractor, whose judgment is to be based on a merely superficial examination.

In Fig. 1 is shown graphically the relative volumes of the materials and number of lines given them in the specifications.

Yet despite the disparity in volume to the materials going to make the finished product, the manufacturer of the cement is always held morally, if not financially, responsible for any failure of the concrete or mortar to properly harden and we have yet to find a case of such failure which was not at first ascribed to poor cement, and it is only after the most rigid investigation that the cement is given a clean bill of health. This being the case, is not the cement manufacturer justified in demanding of the engineer such inspection and tests of the materials forming the aggregate as will insure their proper action in conjunction with the cement, and can we fairly refuse it?

At the risk of boring you with figures, we cite three typical cases coming directly under our observation of failure of the cement to harden which, on investigation, was found to be due entirely to the sand used.

Case A.—A client reported that in spite of the cement used on the work having passed the inspection of the laboratory of one of the authors, that it failed to harden when made into concrete and that it would be necessary to tear out about one hundred yards of pier foundations for a bridge that they were building. On looking up the test the cement was found to be normal, having given, when tested with standard sand, the following results:

Setting time.	Per cent. of water, 23.
Initial set, 4 hrs. 10 min.	Temperature of air, 63° F.
Final set, 7 hrs. 20 min.	Temperature of water, 60° F.
Constancy of volume.	
Cold water pat, good.	
Air pats, good.	Specific gravity, 3.14.
Steam test, good.	

Tensile test. Age, 7 days.

Neat. Per cent. of water, 23.	1 part cement to 3 parts sand. Per cent. of water, 9.8.
600 lbs.	200 lbs.
625 "	225 "
613 "	220 "
587 "	218 "
629 "	222 "
Av. 610 lbs.	Av. 217 lbs.

Tensile test. Age, 28 days.

Neat. Per cent. of water, 23.	1 part cement to 3 parts sand. Per cent. of water, 9.8.
738 lbs.	325 lbs.
725 "	320 "
727 "	318 "
728 "	347 "
737 "	310 "
Av. 731 lbs.	Av. 324 lbs.

Inspection of the work, however, showed the concrete to be absolutely worthless, being entirely without bond, there being no more adhesion of one stone to another than if clay had been used.

On examination of the sand or fine gravel used it was found to contain a number of fine particles and small pebbles of reddish brown color, which, on being moistened and rubbed together, gave a paint-like substance of dark red color and to this we attributed the failure of the cement to harden, and suggested the use of another sand in the construction work pending thorough test of the original sand. To this the contractor grudgingly assented, stating that he had used the sand before with most satisfactory results, and that, all tests to the contrary, the fault lay with the cement and that he would substitute another brand of cement as well.

Briquettes were made from the gravel and cement first used, the original cement and substituted sand and the original cement and Ottawa sand, in the proportions of one cement to three sand, standard methods being followed in each case, which, on being broken at seven and twenty-eight days gave the results shown in Table I.

TABLE I.—COMPARATIVE TESTS OF SAND MIXED WITH CEMENT IN THE PROPORTION OF ONE PART CEMENT TO THREE PARTS SAND.

Original sand.	Substituted sand.	Ottawa sand.
7 days.	7 days.	7 days.
392 lbs.	169 lbs.	228 lbs.
394 "	159 "	218 "
420 "	158 "	223 "
... "	... "	220 "
... "	... "	215 "
Av. 402 lbs.	Av. 161 lbs.	Av. 220 lbs.
28 days.	28 days.	28 days.
504 lbs.	243 lbs.	408 lbs.
507 "	238 "	410 "
485 "	229 "	384 "
500 "	246 "	390 "
... "	... "	409 "
Av. 499 lbs.	Av. 234 lbs.	Av. 400 lbs.

The suspected sand exceeded in tensile strength the Ottawa sand and the substituted sand at both seven and twenty-eight days.

As a consequence of this test the original sand was ordered to be again used in the work by the engineer directly in charge, but its use was immediately followed by a recurrence of the original

TABLE II.—COMPARATIVE TEST OF CONCRETE MADE IN THE PROPORTION OF ONE PART CEMENT, THREE PARTS SAND AND FIVE PARTS CRUSHED STONE.

Blocks " A " were made from material taken from the concrete mixer.
Blocks " B " were made by hand and were fairly dry concrete.

Blocks " A."

Age	Size.	Failed at
44 days	6 x 6 x 6 ins.	323 lbs.
44 "	6 x 6 x 6 ins.	642 "
		Av. 483 lbs.

Blocks " B."

Age.	Size.	Failed at
7 days	6 x 6 x 6 ins.	24,030 lbs.
7 "	6 x 6 x 6 ins.	24,160 "
		Av. 24,095 lbs.
28 days	6 x 6 x 6 ins.	31,660 lbs.
28 "	6 x 6 x 6 ins.	32,770 "
		Av. 32,315 lbs.

trouble, although another cement was being used. Twenty concrete blocks were then made from the concrete mixture on the work, four blocks being taken from five different batches as it left the mixer and for comparison of these, test cubes were made in the laboratory from the same materials which were mixed by hand. Of the twenty blocks from the work only two gained sufficient strength to stand the jar of transportation and hold together sufficiently to be placed on the testing machine. The results of the comparative test are given in Table II.

Further investigations showed that the dark particles in the gravel were nothing but rounded pieces of shale, which on being placed in the concrete mixer, which was of the batch type, with the coarse aggregate and an excess of water, were ground to clay, the action being identical with that of a tube mill. Tensile tests of mortar using the original sand and cement were then made under conditions more nearly approaching those on the work than those prescribed by the Standard Methods for Testing Cements. The cement was mixed wet, placed in a vessel and stirred rapidly for five minutes after the water had been added and on being broken the briquettes gave the following results:

One part Original Cement to three parts Original Sand.

	7 days.	28 days.
	112 lbs.	234 lbs.
	110 "	258 "
	105 "	217 "
	107 "	244 "
Av.	108 lbs.	238 lbs.

These briquettes did not harden sufficiently in the 24-hour period to be removed from the molds and had to be kept in the mold 48 hours before hardening, after which they were put in water.

Case B.—One of the authors received from a client a letter reading as follows: "We have had trouble with cement setting up in some work we are doing and enclose two samples marked No. 1 and No. 2. The sample of cement marked No. 1 is the one we had the most trouble with. Sample No. 2, which is another brand, is the one we are now using and, while it works a little better, the strength of the concrete is far from satisfactory. Will you kindly determine what is the matter with the cement?"

Tests made on these cements, in accordance with the standard specifications, Ottawa sand being used, gave the following results:

Setting time.	Cement No. 1.	Cement No. 2.
Initial set	3 hrs. 47 min.	2 hrs. 20 min.
Final set	5 hrs. 8 min.	4 hrs. 15 min.
Steam test	Good	Good

Tensile strength, 1 part cement to 3 parts sand. Age, 7 days.

Cement No. 1.	Cement No. 2.
240 lbs.	215 lbs.
235 "	200 "
230 "	235 "
260 "	230 "
240 "	210 "
Av. 241 lbs.	Av. 218 lbs.

These results showed both cements to be of good quality and normal strength. In reporting these tests it was suggested that the trouble might be with the sand, and a sample of the sand used in the work was sent for tests, as well as samples of two other sands which could be substituted. These sands were marked No. 3, No. 4, No. 5, the letter accompanying the sands reading as follows: "The sample of sand No. 4 is taken from the premises and is what we have been using, and, like you, we are beginning to be very suspicious of it as rough tests made with the cement and the two samples of sand sent you, show them to harden perfectly. The sample No. 5 is from a pit near the work and we would like to use it if suitable as it would be much cheaper. The sample of sand No. 3 is from another pit at some distance away and is used very extensively, and we feel sure of it but it will cost almost double that of No. 5. Kindly make comparative tests with cements No. 1 and No. 2."

Comparative tests were made of these sands, the standard method being followed in each case. The results are given in Table III.

The tests were not carried far enough to show the reason for the failure of sand No. 4 to make good mortar. To the eye all the sands were similar and the mechanical analysis showed sand No. 3 to be much finer than sand No. 4 which approached nearest to the Ottawa sand in the size of the grains, yet sand No. 3 gave very good strength. The percentage of loam was low in all samples.

TABLE III.—COMPARATIVE TESTS OF SAND NO. 3, NO. 4, AND NO. 5 WITH CEMENTS NO. 1 AND NO. 2.

	Sand No. 3.	Sand No. 4.	Sand No. 5.
Per cent. of loam	0.2	2.0	3.0

Mechanical Analysis.

Size of sieve.	Sand No. 3.	Sand No. 4.	Sand No. 5.
	—Per cent. passing previous sieve— retained on sieve.		
2	0.0	0.0	0.0
4	0.0	1.0	3.0
6	0.2	0.1	2.2
10	1.2	1.2	4.3
20	13.9	24.6	16.4
30	15.1	45.3	21.3
40	11.9	14.3	19.3
50	13.3	7.8	16.2
80	18.2	3.6	15.2
100	8.4	0.6	1.7
200	11.6	0.4	1.3
Passing 200	6.2	0.1	0.1

Tensile test. Cement No. 1, 1 part cement to 3 parts sand. Age, 7 days.

Sand No. 3.	Sand No. 4.	Sand No. 5.	Ottawa sand.
175 lbs.	No strength	175 lbs.	240 lbs.
170 "	" "	170 "	235 "
195 "	" "	160 "	230 "
190 "	" "	170 "	260 "
170 "	" "	175 "	240 "
Av. 180 lbs.		Av. 170 lbs.	Av. 241 lbs.

Tensile test. Cement No. 2, 1 part cement to 3 parts sand. Age, 7 days.

Sand No. 3.	Sand No. 4.	Sand No. 5.	Ottawa sand.
205 lbs.	75 lbs.	215 lbs.	200 lbs.
195 "	95 "	200 "	290 "
195 "	90 "	235 "	380 "
180 "	85 "	230 "	340 "
190 "	90 "	210 "	330 "
Av. 193 lbs.	Av. 87 lbs.	Av. 218 lbs.	Av. 314 lbs.

Case C.—A contractor reported that on some work he was doing the cement was bad, a portion of the work failing to set up although portions of the concrete were satisfactory. Samples of the cement were secured and tested with standard Ottawa sand, and the result showed the cement to be normal in every way, the average of the several tests, thirty-five samples of cement in all, being taken, was as follows:

AVERAGE OF TENSILE TESTS OF SUSPECTED CEMENT AND OTTAWA SAND.

Setting time.	Fineness.
Initial set, 265 min.	Passing No. 100 sieve, 94.3%
Final set, 411 min.	Passing No. 200 sieve, 77.7%

Constancy of volume.

Air pat, good.	Steam test, good.
Cold water pat, good.	Specific gravity, 3.19.

Tensile test.

Age.	Neat.	1 part cement to 3 parts sand.
24 hours.	391 lbs.	000 lbs.
7 days.	663 "	283 "

Three samples of the sand used on the work were also taken and marked "A," "B," and "C" for identification. To the eye and touch the sand appeared of good quality, being clean and sharp, well graded as to size of grains and free from loam and dirt. The general appearance of the sand was such that it would have been accepted on a superficial examination by most engineers without question.

A portion of the Samples A and C were made into briquettes with a composite sample of cement made by taking equal parts by weight from the individual samples. A third set was also made from Ottawa sand for comparison.

COMPARATIVE TENSILE TESTS OF SAND AND CEMENT FROM THE WORK AND OTTAWA SAND, USING ONE PART CEMENT AND THREE PARTS SAND.

Age.	Sand A.	Sand C.	Ottawa Sand.
7 days.	61 lbs.	40 lbs.	266 lbs.
28 "	128 "	....	407 "

Sample B was not tested as the supply of the sand was limited.

A comparative test was made, using Sands A, B, and C and Ottawa sand and another brand of cement that had been previously

tested and found to pass the requirements of the standard specifications.

COMPARATIVE TESTS OF SANDS IN QUESTION, OTTAWA SAND AND NORMAL CEMENT.

Age.	Sand A.	Sand B.	Sand C.	Ottawa Sand.
7 days.	21 lbs.	78 lbs.	22 lbs.	250 lbs.
28 "	130 "	....	....	438 "

These tests clearly show the fault to be with the sand and not with the cement and closed the investigation as far as the commercial side was involved. It was, however, decided to extend the test and try to determine the cause of the abnormal action. The sands were first subjected to mechanical analysis by sieving to determine the fineness and gradation of the sand. As will be seen by the table there was practically no difference in the three samples as far as size of grains was concerned.

MECHANICAL ANALYSIS OF SANDS A, B, C, AND SYNTHETICAL SAND.

Size of sieve.	Size of mesh, m.m.	Per cent. of sand passing previous sieves and retained on sieve. A.	B.	C.	Synthetical Sand.
10	1.8542	35.9	36.1	32.1	39.0
20	.8509	20.0	20.1	18.4	18.7
30	.53467	14.3	15.9	13.9	11.0
40	.37465	9.7	11.2	10.8	9.7
50	.2794	9.3	7.2	9.4	9.3
60	.2327	3.8	3.6	4.8	5.0
80	.17145	3.2	3.2	5.5	3.1
100	.1397	1.2	1.2	2.0	1.5
Per cent. passing No. 100		2.5	1.5	2.9	2.7

In order to prove conclusively that the granulative ratio of tne sand was not responsible for the failure of the cement to harden, a synthetical sand was made of approximately the same granular composition from pure quartz; this on being tested with the normal cement used with Sands A and C previously mentioned gave the following results:

COMPARATIVE TENSILE TESTS OF SAND A, SYNTHETICAL SAND, AND STANDARD OTTAWA SAND.

Age.	Sand A.	Synthetical Sand.	Ottawa Sand.
7 days.	21 lbs.	295 lbs.	250 lbs.
28 "	130 "	478 "	438 "

These results proved conclusively that the failure was not due to the granular composition.

The mineral analysis of the sand was then made:

MINERAL ANALYSIS OF SAMPLE OF SAND A.

Sand quartz	51.12%
Clay substance	7.78%
Feldspar	36.12%
Mica	2.50%

This shows the sand to consist of about one-half quartz grains, the remainder consisting of partially decomposed granitic rock. To the mica and softer clay matter was attributed the failure of the sand to make good mortar.

To demonstrate this a sample of sand was washed, removing the readily soluble materials and part of the mica, this when made into briquettes one part cement to three parts sand, with the same normal cement previously used, gave much better tensile tests.

COMPARATIVE TEST OF SAND A WASHED AND UNWASHED, OTTAWA SAND AND NORMAL CEMENT.

Age.	Sand A Unwashed.	Sand A Washed.	Ottawa Sand.
7 days.	21 lbs.	172 lbs.	250 lbs.
28 "	130 "	228 "	438 "

These three cases show the maximum effect of sand on the strength of cement mortars and concrete. Such extreme cases, however, are not frequent and from an engineering standpoint are less dangerous than where the lack strength due to the sand is less marked. When the failure to harden is complete the effect is so obvious as to insure the taking out of the defective material but where the failure to harden is only partial, it may not be discovered during construction, in which event the latent weakness in the structure may in time of unusual stress cause failure and consequent grave disaster.

Table IV shows the tensile tests of various commercial sands tested in comparison with standard Ottawa sand, the methods prescribed for testing cement in the standard specifications being followed. The results given in each case show the average of five briquettes; where several sands are bracketed together the results are directly comparative, the same cement being used.

Table V is calculated from the tests shown in Table IV. To eliminate variations due to the cements the same letters refer to the same sands.

TABLE IV.

COMPARATIVE TENSILE TESTS OF COMMERCIAL SAND AND STANDARD OTTAWA SAND.

Tensile strength in lbs. per sq. in.

	Commercial Sand. 1 cement, 3 sand.		Standard Sand. 1 cement, 3 sand.	
	7 days.	28 days.	7 days.	28 days.
A........	135	207	319	489
B........	369	395	319	489
C........	211	313	339	518
D........	327	445	339	518
E........	163	329	339	518
F........	230	306	338	514
G........	207	282	362	441
H........	104	143	310	441
I........	166	243	310	441
J........	154	329	310	441
K........	402	499	220	400
L........	21	130	266	407
M........	271	363	327	447
N........	85	232	160	268
O........	191	301	329	447
P........	182	284	329	447
Q........	295	478	266	407
R........	135	209	327	447
S........	129	205	253	363

Sands A, E and I are commercially known as Philadelphia Bar Sand.
C—Jersey gravel.
H—Susquehanna Bar Sand from Harrisburg.
J—An artificial sand made by crushing a very fine-grained sandstone.
F—From New England.
D—Bar Sand.
K—Gravel from small river flowing through shale formation.
L—Sand formed from decomposition of granite rock.
M—Cowbay sand from Long Island.
N—Sand from California of granitic origin.
O—Sand from Hornos, Mexico.
P—Sand from Gomez Palacio, Mexico.
Q—A synthetical sand made from pure quartz.
R—Sand from California of granitic origin.
S—Unknown.
The sources of B and G are unknown.
Sands K, L, N and R all proved unsatisfactory in actual work.

The first column of figures gives the per cent. of strength of standard sand, the commercial sand developed.

The second column shows the calculated breaking strength at seven days, obtained by multiplying the average strength per square inch of briquettes, made from Ottawa sand and various commercial cements in the proportions of one cement and three sand. This average strength is 253 lbs. per sq. in., and was arrived at by averaging the seven-day tests, one cement and three sand, in the laboratory of one of the writers for the past year.

The third column shows the per cent. of strength of standard sand developed by the commercial sand at 28 days.

The fourth column shows the calculated breaking strength at 28 days, obtained in the same manner as in the seven-day test. The factor used for the strength of standard Ottawa sand briquettes for this period being 368 lbs. per sq. in.

TABLE V.

	7 days		28 days	
	Per cent. of strength of standard sand developed.	Calculated equivalent value. Lbs. per sq. in.	Per cent. of strength of standard sand developed.	Calculated equivalent value. Lbs. per sq. in.
A	42	106	42	154
B	115	291	80	294
C	62	156	60	220
D	96	242	86	316
E	48	121	63	231
F	68	172	60	220
G	57	144	64	235
H	34	85	32	118
I	53	134	55	202
J	50	126	74	272
K	182	460	124	456
L	08	20	31	113
M	80	202	80	294
N	53	134	80	294
O	58	146	74	272
P	55	139	64	235
Q	110	278	110	404
R	40	101	40	147
S	50	126	56	206

Table VI shows the granulometric composition of various commercial sands physical tests, one cement and three sand, and comparative tests with standard Ottawa sand.

TABLE VI.

Mark.	Kind.	Per cent. by Weight Passing Previous Sieve and Retained on Sieve Number. 4	10	20	30	40	50	60	80	100	200	Passing 200 Sieve.	Tensile Strength. Lbs. per Square Inch. 1:3 Mortar. 7 Days	28 Days	90 Days	180 Days	270 Days	One Year	Tensile Strength. Lbs. per Square Inch. Standard Ottawa Sand. 1:3 Mortar. 7 Days	28 Days	90 Days	180 Days	270 Days	One Year
3	Bank	0.0	1.4	13.9	15.1	11.9	13.3	.....	18.2	8.4	11.6	6.2	180	....	....	....			241					
*4	"	1.0	1.3	24.6	45.3	14.3	7.8	.....	3.6	.6	.4	.1	No sth.	....	....	....			241	...				
5	"	3.0	6.5	16.4	21.3	19.5	16.2	.....	15.2	1.7	1.3	.1	170		....	....			241					
*A	"	0.0	35.9	20.0	14.3	9.7	9.3	3.8	3.2	1.2	2.5	....	21	130	....	....	....		266	407	...			
*B	"	0.0	36.1	20.1	15.9	11.2	7.2	3.6	3.2	1.2	1.5	....	78	....	....	....			266	407	...			...
*C	"	0.0	32.1	18.4	13.9	10.8	9.4	4.8	5.5	2.0	2.9	....	22	....	....	....			266	407				
Q	"	0.0	39.0	18.7	11.0	9.7	9.3	5.0	3.1	1.5	2.7	....	295	478		....	....		266	407				
H	Bar	0.0	1.1	4.4	11.5	9.9	24.3	21.3	21.5	4.5	1.5	....	104	143	246	243	....		310	441	246	482	...	...
I	"	0.0	0.92	2.91	9.05	25.21	36.28	14.54	7.18	1.4	2 51	....	166	243	320	343	....		310	441	320	482	....	...
J	Crushed Stone	6.0	4.8	6.3	6.6	7.20	10.7	9.9	17.2	15.6	20.7		154	329	393	424	....		310	441	393	482	....	....
M	Bank	0.0	16.71	15.65	15.35	20.85	6.82	10.35	4.05	4.01	6.75		271	363		....	....	....	327	447	....	....	....	....
*N	"	12.89	23.87	26.13	12.45	7.82	6.53	3.29	3.87	0.0	3.15	....	85	237	424	....	....	....	160	268	....	....	....	....
*R	"	0.0	0.2	3.55	21.0	23.90	24.90	.....	21.7	3.5	1.25	....	129	205	....	....	....		253	363	....	....	....	
Sand 1	Bar	0.0	3.2	10.3	29.3	29.8	5.7	.....	2.2	3.4	14.8	.9	390	480	473	415	...	368	213	453	510	433	...	457
" 2	"	0.0	5.4	25.4	26.0	25.7	12.8	.....	4.2	.4	.1	...	343	435	491	369	...	320	"	"	"	"		"
" 3	"	0.0	24.7	23.2	15.0	13.2	9.4		6.6	1.9	1.9	3.3	392	522	515	495	...	565	"	"	"	"	.	"
" 4	Bank	0.0	33.0	34.9	15.2	8.4	4.4	.....	2.1	.4	.3	.9	521	618	755	731	..	773	"	"	"	"	.	"
" 5	Bar	0.0	18.5	22.7	19.7	23.4	11.4	.....	3.3	.3	.1	....	410	499	555	420	.	415	"	"	"	"	..	"
" 6	Bank	0.0	15.5	15.9	16.0	20.5	17.7	.....	9.1	1.7	1.9	1.2	490	622	705	614	.	754	"	"	"	"	.	"
" 7	"	0.0	7.4	14.4	17.0	23.3	21.4	.....	12.0	1.9	1.1	.7	417	527	519	603	.	635	"	"	"	"		"
" 8	"	0.0	30.7	22.2	14.2	13.7	10.7	.....	6.0	.9	.6	.6	545	628	745	720	...	689	"	"	"	"	...	"
" 9	"	0.0	31.1	17.3	12.2	15.0	14.4	.....	6.5	1.0	.8	1.3	401	493	495	483	...	525	"	"	"	"	...	"
" 10	"	0.0	6.4	12.0	14.3	18.6	21.5	.....	18.0	4.6	3.0	1.2	411	547	659	488	....	596	"	"		"	...	"
" 11	Mix. of Sand and Limestone Screenings	0.0	32.4	20.9	9.7	8.1	8.8	.....	10.9	3.3	2.3	3.0	428	602	651	708	....	735	"	"	"	"	....	"
" 12	"	0.0	33.3	24.7	11.8	7.5	7.2	.....	7.3	2.4	2.2	2.8	407	597	622	601	...	806	"	"	"	"		"
" 13	Bank	0.0	30.9	30.4	14.4	10.0	6.7	.....	5.2	1.0	.4	.2	543	585	655	668	....	778	"	"	"	"	...	"
" 14	Bar	0.0	7.4	21.4	12.0	13.3	15.3	.....	15.2	6.7	5.7	2.1	422	444	433	481	...	579	"	"	"	"	....	"
" 15	"	0.0	1.0	1.8	2.0	8.5	25.3	.....	44.7	9.6	4.5	1.2	312	335	374	331	...	377	"	"	"	"	...	"
" 16	"	0.0	39.5	27.5	11.6	9.4	6.8	.....	3.0	.8	.4	.3	471	518	581	605	..	650	"	"	"	"	...	"
" 17	Bank	0.0	8.4	7.5	7.8	14.3	24.2	.....	25.4	6.1	3.6	1.8	343	361	436	466	..	476	"	"	"	"	....	"
" 18	"	0.0	4.1	16.3	16.0	19.0	13.8	.....	16.3	4.1	5.0	4.6	366	477	514	550	...	555	"	"	"	"	...	"
" 19	"	0.0	27.1	26.9	16.4	14.3	7.2	.....	5.2	.8	.6	.7	535	654	675	773	...	807	"	"	"	"	...	"
" 20	"	0 0	23.3	26.5	13.9	11.7	7.7	.....	11.7	1.9	1.3	1.1	440	556	598	670	....	703	"	"	"	"	...	"
" 21	Bar	0.0	1.0	7.3	7.6	20.0	43.5	.....	18.0	1.6	.2	.1	287	356	341	380	....	390	"	"	"	"	...	"
" 22	"	0.0	7.33	23.7	14.3	21.6	15.8	.....	15.1	1.7	.5	.1	277	442	440	487	....	....	"	"	"	"		"

* Sand failed in work.

The letters and numbers used to identify the different sands for the first thirteen tests being the same as used previously in this paper. These tests were made by the writers and the remaining tests marked "S.D.1 to S.D.22" were compiled from "Portland Cement Mortars and Their Constituent Materials," by Richard L. Humphrey and Wm. Gordon, Jr. No direct comparative test is given between these sands and Ottawa sand and the figure used to denote the strength of standard Ottawa sand mortar was the average given for tests of the mixed cement with standard sand, Table No. 2-B, page 18.

In comparing these latter tests with those given in other tables it must be remembered that they were made from cement and sand especially selected for laboratory tests, while the others were made from sand and cement taken from the work and in Table VI Sands No. 4, A, B, C, N, and R, failed in work.

The various tests given in this paper are not presented as an exhaustive research into the action of sand and cement mortars but, on the contrary, are cited with full recognition of their fragmentary character; but we think they prove conclusively that the strength of cement mortars and concrete is dependent as much on the sand as on the cement and show the necessity of careful and systematic inspection and testing of the sand and also the desirability of the development of standard methods of testing and their general adoption so that the work of different engineers may be comparative.

Fully realizing the inefficiency of the data before us and of the necessity of further research and experimental work before a general specification can be drawn that will meet all conditions, it is with some hesitancy that we give the specifications and methods of tests adopted by one of the writers in his work. These specifications and methods of testing being offered as our contributions to what we hope may prove to be a general discussion and investigation of the subject, rather than as a definite statement of what is required to secure the best sand for cement mortars and concrete.

SPECIFICATIONS.

The contractor shall furnish the sand in such manner as to allow of its being tested before being used in the work.

All sand used in the work shall be subject to the following tests:

Size—The sand shall be of such fineness as to all pass a No. 4 screen.

Chemical Composition—The sand shall contain at least 95 per cent. of silica.

Before acceptance of the sand, the contractor shall furnish to the engineers average samples taken from the source of supply, which shall be subject to analyses, and comparative physical tests with standard Ottawa sand.

The sand shall develop, when tested, in the proportion of one cement to three with sand a normal Portland cement according to standard methods, at least fifty per cent. of the strength developed by the Ottawa sand at the seven and twenty-eight-day periods. In no case shall a one to three mixture briquette fail at less than one hundred pounds at seven days or one hundred and fifty pounds at twenty-eight days. The sand when tested with fifty per cent. excess water over that called for by the standard methods shall develop at least seventy-five per cent. of the strength developed when tested according to standard methods.

After full tests have been made to determine the character of the sand, each carload shall be subject to tensile tests and must develop at seven days at least eighty per cent. of the strength shown in the preliminary tests on which it was accepted.

METHODS OF TESTS.

Determination of mineralogical composition of sand more particularly the percentage of mica and clay matter in the sand is a difficult analysis and one for which, to the writer's knowledge, no proper method has been evolved. The clay matter in the sand can be determined by boiling in concentrated sulphuric acid in which the quartz, feldspar and mica are practically insoluble. The determination of the percentage of feldspar and mica remaining in the quartz is not difficult but the separation of the feldspar from the mica is exceedingly difficult, and any methods known are only approximate and attended with considerable possibilities of error.

Sedimentation has been suggested as a method of separating the mica from the feldspar in quartz but the writers do not know of sufficient tests having been made to determine the accuracy of

this method and therefore confines the chemical work to determinations of silica. If, however, a full mineralogical analysis is desired covering clay matter, feldspar and mica, the methods suggested by Seger for clay and Vogt for mica can be used but the results will only be approximate.

For this reason we confine the chemical work to determination of silica alone and specify a high content. Where sand of this character cannot be obtained, modifications would have to be made in the specifications.

PHYSICAL TESTS.

The mechanical analysis of the sand as called for in the specifications is exceedingly simple, requiring only a No. 4 screen. The writer fully realized the value of the work done by other investigators covering the granulometric composition of sands and while those will materially affect the strength of mortars as determined by the ordinary tensile tests, do not think they are of sufficient importance in their effect on the ultimate strength of concrete masses provided the sand gives good physical tests to require special specifications and warrant the increased cost that might be involved in securing a sand of specified granular composition. If it is desired, however, to cover the granulometric composition in specifications, this can be best determined by practically standard methods suggested in the books of Hazen, Ferret and Thompson & Taylor and other authors.

Tensile tests of actual mortars should be made in accordance with the standard specifications for the testing of cement in order to compare the action of the sand and cement under the best conditions with the results obtained with a known sand such as standard Ottawa sand. The writer's experience, however, lead them to believe that these tests cannot be depended upon as indicating the actual action of the sand in the work; this is clearly shown by Sand A in this paper.

A number of similar experiences led the author of the specifications to adopt in his laboratory the following method of testing sand:

Sand to be tested is made into a one to three mortar by weight with a normal cement according to the standard specifications for tests at seven and twenty-eight-day periods; a similar set of briquettes being made in the same manner from standard Ottawa

sand. A second set of briquettes is then made from the commercial sand using fifty per cent. more water than required for normal consistency, the mortar being thoroughly worked for ten minutes prior to being placed in the molds. The effect of so treating sand is indicated by Table VII.

TABLE VII.

Mark.	Tensile Test, Standard Specifications.			Tensile Test, 50 per cent. Excess Water.			Comparative Test, Ottawa Sand, Standard Specifications.		
	Per Cent. of Water.	7 Days.	28 Days.	Per Cent. of Water.	7 Days.	28 Days.	Per Cent. of Water.	7 Days.	28 Days.
Case A*	9.8	402	499	15.	108	238	9.8	220	400
" B*	10.	369	395	15.	79	123	9.	319	489
" C	12.	135	207	18.	118	154	9.	319	489
" D	8.08	319	489	13.2	102	231	9.	319	489
" E*	11.	85	237	16.5	63	157	9.	160	268
" F	10.	271	363	15.	153	290	9.8	327	447
" G	11.	246	306	16.9	203	301	9.8	327	447

* Sands failed in work.

Lately some experimental tests have been made where the sand and cement to be tested is placed in a revolving vessel with flint pebbles, the object being to approach as nearly as possible the conditions obtained in a concrete mixer. These tests, however, have not been carried sufficiently far to admit of judging of the value. Table VIII gives an idea of the results obtained by this last method of testing.

TABLE VIII.

Mark.	Tensile tests, standard method.			Tensile tests, mortar wet and placed in revolving vessel with flint pebbles.		
	7 days.	28 days.	90 days.	7 days.	28 days.	90 days.
A–1	150 lbs.	268 lbs.	... lbs.	90 lbs.	215 lbs.	297 lbs.
B–2	85 "	237 "	424 "	44 "	132 "	223 "

The purpose of this test is to ascertain the effect on the sand of the excess water and grinding action of the concrete mixer. The test was suggested by the fact that sands which gave good strength in laboratory tests failed to make as good concrete as was made from other sands testing lower. This was attributed to the breaking down through the grinding action of the mixer of the softer particles in the sand, increasing the quantity of fine material present and distributing any deleterious substances through the mass.

While it is probable that on long-time tests the wet mixes would approach more nearly the dry mix in tensile strength, the early strength developed is an important element to be considered in selecting sand for reinforced concrete construction.

The whole subject of testing of sands is open to investigation and will require careful experimenting and a number of tests before definite methods can be established.

The chemical action and mineralogical composition of sand should also be carefully studied. That this influences the hardening of mortars is unquestioned but there is no general agreement among writers on this subject. Mica is generally considered detrimental. Mr. W. N. Willis found that the addition of 2½ per cent. of finely ground mica to Ottawa sand reduced the strength of the mortar at twenty-eight days about thirty-three per cent. and that the presence of flakes of mica greatly increased the percentage of voids. (*Engineering Record*, January 28, 1908).

Clay matter, by which is to be understood hydrous silicate of alumina, and which must not be confounded with fine silicious earth, is often detrimental.

Fine silicious particles unless present in large quantities are not detrimental, and may be beneficial with lean mortars and coarse sands as they tend to make a denser and more impermeable mortar.

The exact action of the mica and clay on the hardening of mortars has not been determined; the probabilities, however, are, that the action is entirely physical as there is no known chemical reaction between the mica or clay and Portland cement during hydration.

If the effect is other than physical it must be brought about by some obscure catalytic or electrolytic action which may either alter the speed of crystallization or the size and arrangement of the crystals or effect in some unknown way the colloidal action.

Clay matter, in order to be injurious, must apparently be hydrous, as dehydrated clay in the shape of crushed bricks, etc., has been used successfully for concrete aggregate.

The action of feldspar is not so well defined and it is probably not injurious unless partially decomposed. Crusher dust obtained from crushing hard feldspathic rocks, such as granite, has been successfully used as a substitute for quartz sand; yet as clay is formed largely from the weathering of feldspar its presence in

natural sand to any large extent should always be regarded as a cause of suspicion. Decomposed, or partially decomposed, organic matter is also objectionable.

The nearer the sand approaches in its mineralogical composition to pure quartz the better will be the results obtained, other things such as size, granulometric composition, etc., being equal. It would, therefore, seem wise, until such time as full investigation has shown that the presence of other materials has no injurious effect, to confine ourselves to the use of as nearly pure quartz as can be commercially obtained.

It is apparent that the work involved in the study of the action of sands in mortar and concrete is too complex to be left to individual effort and the work can best be done by the formation of a joint committee representing all interests as was done in the case of cement, and we believe the subject of sufficient importance to warrant your Society in taking the initiative in the matter.

DISCUSSION.

Mr. S. E. Thompson.—I met recently a number of instances of defective concrete caused by poor sand, and some of them most astonishing because the sand, even upon careful inspection and mechanical analysis, would have been passed without question. Mr. Thompson.

In one very recent case the sand was satisfactory in appearance and gave a mechanical analysis almost identical with that of a sand which had been used successfully in an important construction. The proportion of silt as determined by mechanical analysis was not over 3 per cent. passing a No. 100 sieve, nor 2 per cent. passing a No. 200 sieve, and yet the concrete failed to set. Tensile tests of the strength of the mortar at 7 days ranged from 0 to 30 pounds per square inch. Washing the sand, as in the case cited by the authors of the papers, much increased the strength of the mortar, and even drying the sand slightly strengthened the mortar. It was thought that the poor quality of the sand might be due to the peculiar character of the fine material which it contained, and very close examination showed that the silt when the sand was moist was of a greasy nature, adhering to the grains. The silt also contained some mica, although probably not enough to greatly affect the strength. Chemical analysis of the silt in the two sands showed very little difference except that the loss on ignition of the poor sand was extremely high, 20 per cent. in comparison with 10 per cent. from the other sand. The investigation is still in progress, but thus far it would seem that the bad quality of the sand is due to organic matter rather than to clay.

In another case where the concrete took a month or two to harden, while that alongside of it made from the same carload of cement, set within 24 hours, the sand was very fine and reddish in color and of a somewhat greasy appearance.

In still another instance mica may have contributed to the cause of the failure.

In two or three other cases of low strength the chemical composition of the sand was good, but it was so fine that the strength

Mr. Thompson. of the concrete was far below that required for a proper factor of safety.

Notwithstanding these many instances of poor sand the total number is extremely low compared with those where no trouble occurs. The coroner is only called in case of death and sand is rarely tested with care unless the concrete actually fails. Thus there are but few available cases of defective concrete or mortar from which to determine with accuracy the exact causes of failure.

While concrete is frequently of low strength because of too fine sand, it is a matter for congratulation, and a reason for security to those who are building in reinforced concrete, that no cases have been produced where a poor sand has permitted the concrete or mortar to set properly and then to deteriorate so as to cause failure. In other words, if a mortar or concrete made with good cement properly hardens in the first place, a growth in strength can be depended upon as certain. The fact that the worst trouble with sand is always found in the original setting and hardening leads one to conclude that perhaps the very simplest test may be the most important, viz., to make a specimen of mortar or concrete and see whether it sets and hardens properly in the first day or two.

While the speaker does not pretend to have reached entirely satisfactory conclusions with reference to selecting and testing sands, the following rules may well be borne in mind,

1. No sand from a new bank ever should be used for mortar or concrete without first testing its setting qualities with the cement.

2. It is advisable to specify a limiting tensile strength of mortar in 7 and 28 days, and make briquettes with the materials and in the proportions to be used in the structure.

3. If laboratory tests of tensile strength are not available, a rough setting test should be made by mixing the aggregate with the cement in the required proportions, placing the specimens in a moist closet and requiring that the surface shall bear the pressure of the finger nail without indentation within 24 hours.

The coarseness of the sand is of very great importance in relative strength, a fine sand requiring more cement than a coarse one; while in some cases fineness should prohibit its use.

It is difficult to draw accurate conclusions from chemical tests, as it would seem from practical experience that it is more the condition in which the elements are found in the bank than in the actual

chemical composition. This, however, is by no means decided, and to add to the very meagre literature the following analyses of New England sands are presented through the courtesy of Sherman and Edwards of Boston, by whom they were made. Mr. Thompson.

CHEMICAL ANALYSES OF THREE NEW ENGLAND SANDS.

	A.	B.	C.
Silica + insoluble matter (SiO_2) . .	82.14%	86.46%	72.32%
Iron + Alumina ($Fe_2O_3 + Al_2O_3$) . .	10.90%	7.46%	21.08%
Calcium oxide (Lime = CaO)	1.78%	1.72%	3.60%
Magnesium oxide (MgO)	3.09%	2.63%	1.23%

MR. S. A. BROWN.—I should like to ask Mr. Spackman as to the conditions under which these cubes, or test specimens, were stored previous to testing. The environment has, of course, a great deal to do with the results of the tests. Mr. Brown.

MR. H. S. SPACKMAN.—The test pieces made on the work were stored in as nearly the condition of the actual work as possible. They were made in wooden boxes and placed alongside of the pier and covered with earth. The ones in the laboratory were made in wooden boxes and left in the cellar. Mr. Spackman.

I should like to say in connection with Mr. Thompson's remarks in reference to vegetable matter that I think it has a very decided action. I reached the same conclusion as Mr. Thompson and tried some experiments only recently, adding to the water used in gauging the mortar, starch, equivalent to 0.5 and 1 per cent. of the total mass and it showed a marked effect on the strength of the mortars, as may be seen from the following results:

TENSILE TESTS SHOWING EFFECT OF DISSOLVING STARCH IN WATER USED IN GAUGING.

Time.	No Addition.	0.5% of Mass.	1% of Mass.
7 days	266 lbs.	158 lbs.	142 lbs.
28 days	413 lbs.	270 lbs.	254 lbs.

It is possible that the partial decomposition of vegetable matter may result in leaving a substance producing a colloidal effect similar to that of the starch.

The whole matter should be carefully worked out, starting with synthetical sands made from pure quartz and trying the effect of adding different vegetable matter found in sand, before we can reach any definite conclusions.

Mr. Brown. MR. BROWN.—Permit me to ask Mr. Spackman if in his investigations he considered the effect of the proportion between the fine and coarse particles of the sand. If some of the particles are particularly fine and the others coarse and uniform, the fine particles will act with the cement practically as a mortar so that the strength will vary according to the proportion of these fine particles.

Mr. Spackman. MR. SPACKMAN.—I would answer that question by saying that in this instance tests were made on the sand in its natural condition and after screening, using for the second test only that which would pass a $\frac{1}{4}$-in. mesh, and that both acted badly, which seems to negative the thought that failure was due to the granulometric composition of the sand. This is further indicated by the fact that in another case of failure, we made up an artifical sand from pure quartz of approximately the same granulometric composition as the sand that failed and it gave very good results, exceeding those obtained with Ottawa sand.

Mr. Humphrey. MR. R. L. HUMPHREY.—I do not wish to prolong this discussion but since it is possible that many misleading ideas may be gathered from the paper just read I think it is well to have recorded the views of those who may have information bearing on the subject. As you all know, we have carried on many investigations of sand at the laboratories in St. Louis covering sands from many parts of the country. We have been particularly careful in analyzing the sand to ascertain whether there were any great quantities of organic or vegetable matter present. We have failed to find even among bank sands, where you would expect to find large percentages of organic matter, more than one per cent. of such matter. I think that the character of the water used has a great deal to do with the strength of the mortar. Another cause is extreme fineness of the sand. Only recently I had occasion to inspect some concrete in which a natural gravel had been used. It was observed that in some cases the concrete was quite hard while in others quite soft. Investigation showed that out of the same batch portions would be quite soft while others were hard. The cement failed to reveal any imperfection. In examining the bank from which the gravel was taken it was noticeable that it contained particles up to two or three inches in diameter and from that down to the finer size. There were also strata of very fine sand of the nature of quicksand running through the bank. In

mixing this concrete a great deal of water was used and the result was that when the concrete was discharged from the mixer the large particles came out first and the finer material last. The mixer discharged into a bucket which was handled by a derrick and the contents dumped into the forms. The result was that the larger coarser particles were found in one portion and the finer mortar in others. The extreme fineness of the sand and the extreme wetness of the concrete caused the soluble salts in the cement to come to the surface, forming what is generally known as "laitance," which hardens very slowly, while the other concrete, which was free from this condition, hardened more rapidly. It should be noted, however, that all the concrete hardened eventually. **Mr. Humphrey.**

SOME TESTS OF REINFORCED CONCRETE BEAMS UNDER OFT-REPEATED LOADING.

By H. C. Berry.

These tests were undertaken in the Materials Testing Laboratory of the University of Pennsylvania by the writer, with the assistance of Mr. A. T. Goldbeck, Instructor in Civil Engineering, to study the effect of a great number of repetitions of working loads on the strength and elastic properties of concrete beams. For this purpose the beams were made in duplicate. One was subjected to many thousand repetitions of a load sufficient to cause stresses somewhat greater than are allowed in good practice, and then tested to failure the load being applied and released several times after each increment was added. The duplicate beam was tested to failure in the usual way by students as a part of their regular laboratory work. The load was released but once or twice in these tests.

The beams were standard 8 x 11-in. by 13-ft. beams. They differed only in the kind and amount of the reinforcing steel. Plain and deformed steel was used. Corrugated and Diamond bars of mild steel were furnished by the manufacturers for these tests. Table I gives the results of tests on the steel.

Table I.—Tests of Steel in Beams.

Bar.	Yield Point. Lbs. per sq. in.	Ultimate Strength. Lbs. per sq. in.	Elongation in 8 ins.	Reduction of Area.
Corrugated ...	48,000	81,600	16%	20%
Diamond	53,000	88,000	17%	24%
Plain	38,000	56,000	33%	60%

The concrete was made of 1 part Giant Portland cement, $1\frac{1}{2}$ parts bar sand, and $4\frac{1}{2}$ parts of crushed granite passing a $\frac{3}{4}$-in. sieve. This proportion gave greater density and strength for these materials than the more usual 1:2:4. The concrete was mixed medium wet and troweled into place by hand without the use of heavy rammers. The mold was removed in from one to three

days and the beam covered with damp sawdust. Table II gives data from tests of cubes made from four of the mixes.

TABLE II.—TESTS OF SIX-INCH CUBES.

Concrete from Beam No.	No. of Cubes.	Age, Weeks.	Average Ultimate Strength. Lbs. per sq. in.	Extreme Variation.
2	3	6	2,600	14%
3	3	6	2,290	3%
6	2	6	2,350	4%
7	2	6	2,670	1%

The average ultimate strength of all cubes tested was 2,460 lbs. per sq. in. Tests of five cylinders 8 ins. in diameter and 16 ins. long gave at six weeks an ultimate strength of 1,630 lbs. per sq. in. The initial modulus of elasticity was less than 1,000,000 lbs. per sq. in.

With two exceptions all beams were tested to failure at six weeks. Those subjected to repeated loading were placed on the apparatus at four weeks and given from 200,000 to 400,000 applications of a load, varying from 4,000 to 6,000 lbs. for the different beams. They were then tested to failure that the results might be compared with those of the beams allowed to harden for the full six weeks without being disturbed. The last beam tested received about 1,140,000 applications of the load and was not tested to failure till about eight weeks old.

As shown in Figs. 1 and 2,* the apparatus for the repetition of the load consisted of a pair of proving levers, special bails for the weights, a frame on which were connections to a wrist pin attached to a brick rattler to raise and lower the weights, and a reinforced concrete base to support the beam under test and to develop the reactions. The proving levers were but slightly modified from the regular apparatus furnished by Riehlé Bros. for the calibration of testing machines to 20,000 lbs. The bails were so attached to the levers as to permit of lifting one while the other rested permanently on the lever. This gave control of the maximum and minimum pressures. The connections consisted of rods, turnbuckles and bell cranks. With the exception of the proving levers the apparatus was made in the shop of the Civil Engineering Department.

* Acknowledgment is made to the *Engineering Record* for the cuts used in this paper.

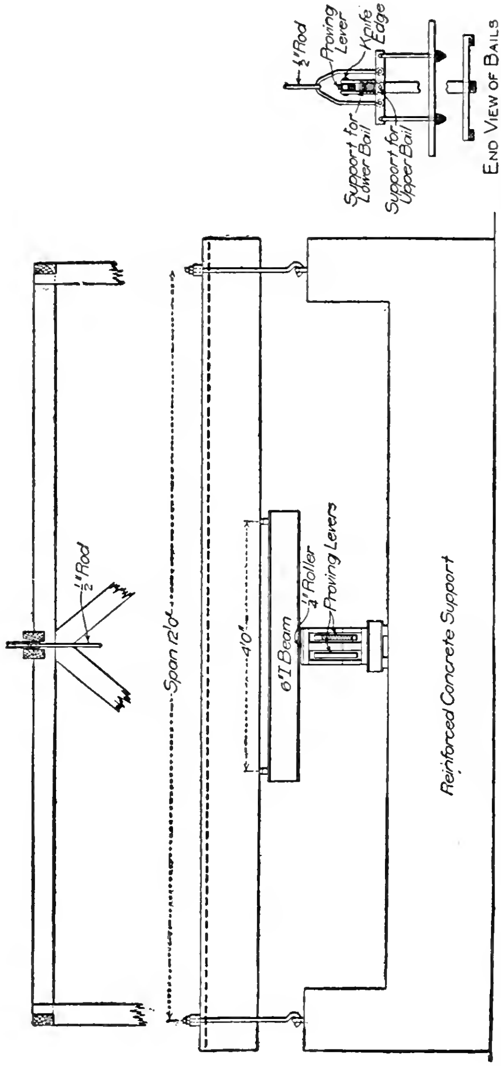

Fig. 1.—Machine for Repeated Applications of Load to Concrete Beams. Side View.

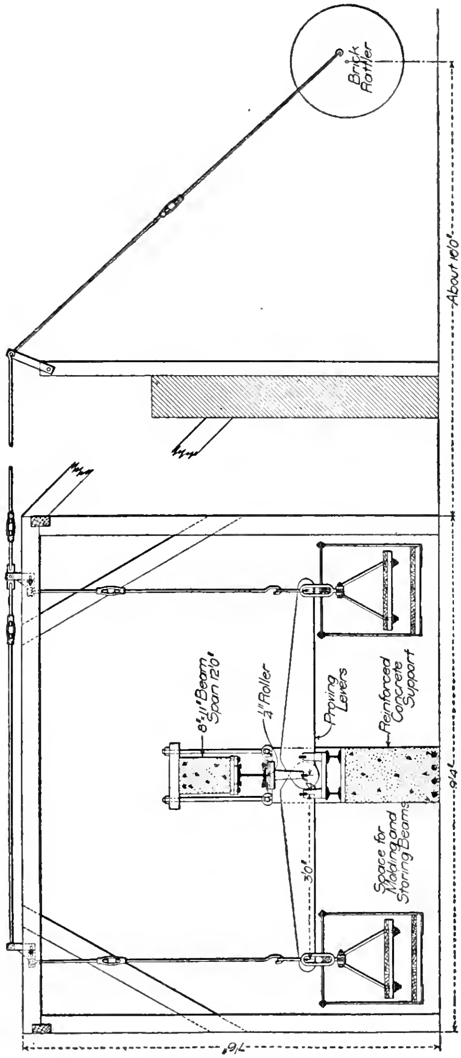

FIG. 2.—End View of Machine.

The beam under test was placed on the apparatus tension side up. This was of material advantage in making examinations for hairline cracks. The load was applied at the third points of the span, which gave zero shear and uniform bending moment over the middle third of the beam where the fiber extensometers were attached. The load in Table III and in the diagrams refers to

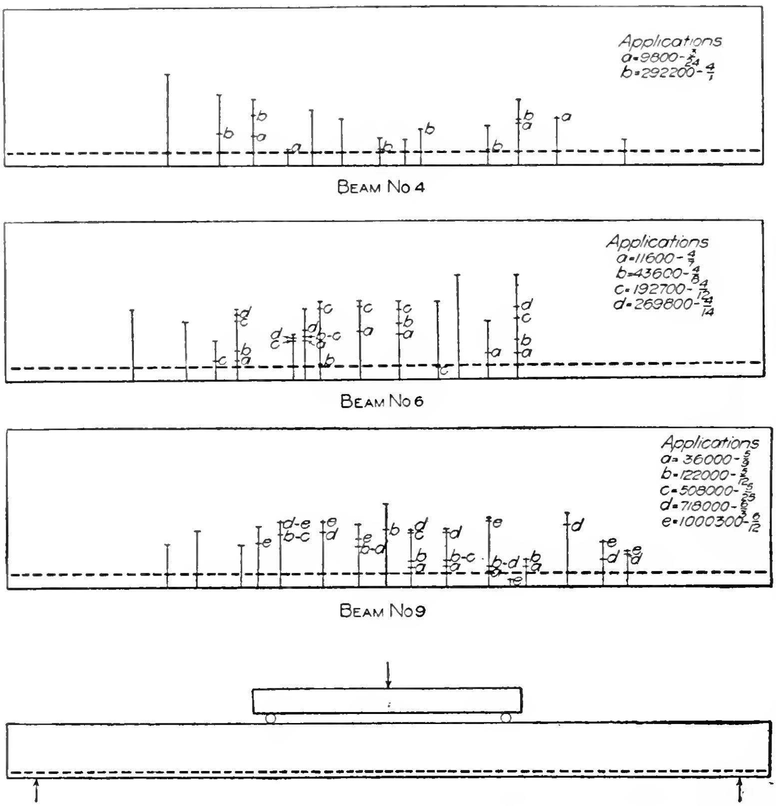

FIG. 3.—Position and Change of Depth in Cracks under Repetition of the same Loading.

the load applied and released by the movable bails, the beam and attached apparatus being supported by the uplift due to the weights in the outer bails which rested permanently on the levers.

The normal speed of the brick rattler is 30 r.p.m. In trying out the apparatus this was reduced to 14 r.p.m. to avoid the effects

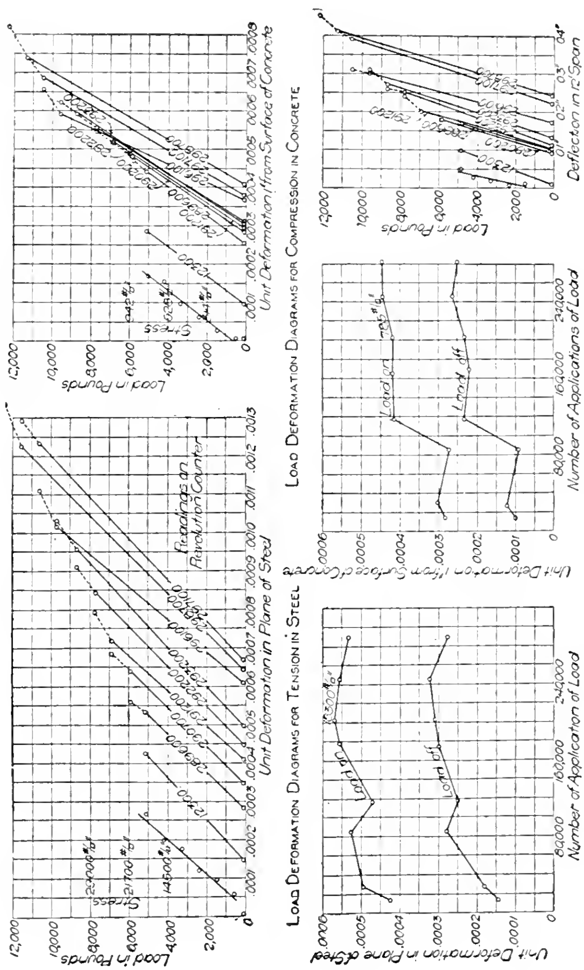

FIG. 4.—Results of Test on Beam No. 2.

of momentum in the application of the load, but it was found that the same object could be attained by a close adjustment of the throw of the lifting rods to the deflection of the levers. Thirty repetitons per minute were made throughout the tests. No attempt was made to determine the effect of varying the rate. The extreme movement of the levers, the deflection of the beam, and the fiber deformations as observed on dial extensometers

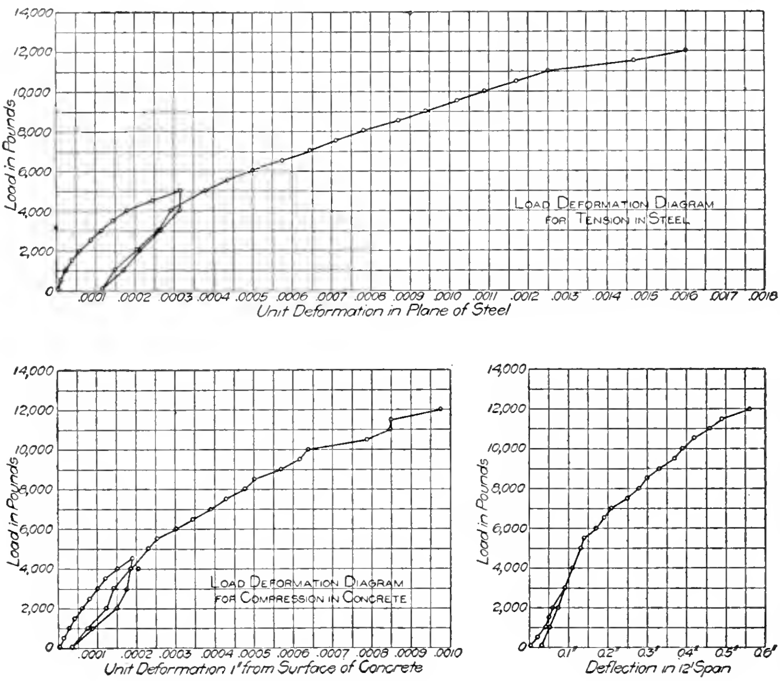

FIG. 5.—Results of Test on Beam No. 3.

temporarily attached were the same when the apparatus was running 30 r.p.m. as when it was moved very slowly by hand. The number of applications was recorded by a revolution counter. The numbers on the diagrams are the readings on the counter and not the number of repetitions between plotted readings.

The deflections were read to the nearest 0.01 in. on a scale fastened to the beam at the middle of the span by the movement of a wire stretched between points at the ends of the span.

Micrometer screw apparatus reading to 0.0001 in. was used to measure the fiber deformations over a 24-in. gauge length in the plane of the steel and in a plane 1 in. below the surface of the concrete on the compression side of the beam. This apparatus is better adapted to tests of this kind than the friction dial type used in the regular laboratory work, because its readings are not affected by vibration and not liable to "creep" on account of the continued back and forth motion. Its zero remained fixed from day to day. No temperature corrections have been made in computing unit deformations. Readings of temperature at the time of observing the deformations for May and part of June showed a mean variation of less than 2° F.

The quite noticeable effects of the repetitive loading are best studied on the diagrams, Figs. 4 to 6, but Table III certainly indicates that the ultimate strength and maximum deflection are little affected by repeated loading to the extent to which these tests were carried.

TABLE III.—SUMMARY OF TESTS SHOWING MAXIMUM LOADS AND DEFLECTIONS.

Beam No.	Reinforcement	Repetition Test. Total Load, Lbs.	Unit-Stress. Lbs. per sq. in. Steel.	Concrete.	Number of Applications.	Ultimate Lead. Lbs.	Maximum Deflection. Ins.
1	3 plain rods, 3/4 in. diam.	*	......	......	200,000	15,000	....
2	4 " " 1/2 " "	5,070	18,300	785	207,000	12,300	0.48
3	4 " " " " "	......	......	......	2	12,000	0.56
4	3 Cor. bars, 3/4 " "	5,070	10,800	940	205,000	17,000	0.55
5	3 " " " " "	......	......	......	1	20,000	0.66
6	2 plain rods, 5/8 " sq.	4,170	15,200	628	305,000	10,500	0.46
9†	2 Diam. bars, 3/4 " diam.	4,170 5,070	14,300 17,100	785 940	718,000 422,000	 13,600	.... 0.78
10	2 " " " " "	......	......	......	3	13,000	0.62

No duplicate to Beam 6 was made. Beam 1 was used in trying out the apparatus and was not subjected to the same load throughout the test. It failed in diagonal tension.

The deflection diagrams show an increase in the deflection for

* From 2,370 to 5,970 during the adjustment and trying out of the apparatus.

† After 718,000 applications of 4,170 lbs., the load increased to 5,070 lbs., and the test continued.

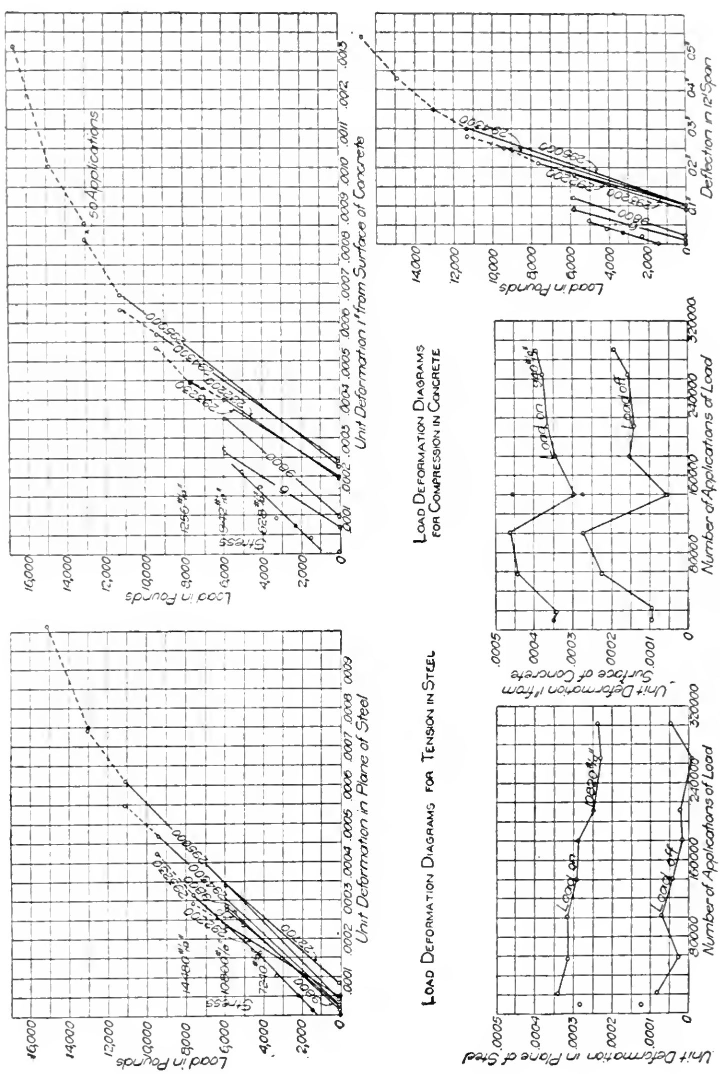

FIG. 6.—Results of Test on Beam No. 4.

the same load as the number of repetitions is increased. The elastic deflection of any beam for a definite load remains nearly constant, but there is an increase in the permanent set. The greater part of this set occurs on the first application and release of the load.

In case of Beam 9, for example, 62,000 repetitions of 4,170 lbs., increased the set in the deflection from 0.04 in. to 0.08 in., while the duplicate beam, No. 10, had 0.05 in. set on the release from the first application of 5,000 lbs. Then 654,000 more applications of 5,070 lbs. on Beam 9 increased the set from 0.08 in. to 0.14 in. and 282,000 more applications of 5,970 lbs. increased it to 0.16 in.

For Beams, 2, 4, 6 and 9, at least one-third of the set present after from 300,000 to 1,000,000 repetitions had occurred before the first 10,000 had been made. No doubt the greatest part of this was due to the first few loadings; for instance, in case of Beam 4 six applications of the load gave one-fourth as much set as 390,000 later applications of the same load.

While it is evident that the rate of increase in the set is relatively very large for the first few applications of the load, there is nothing to indicate that for a working load the set would cease to become greater. In a paper read before the Society last year, Professor W. K. Hatt reported the same phenomena for deflection under repeated loading. Professor Van Ornum's tests, published in the 1907 "Proceedings" of the American Society of Civil Engineers, show a similar increase in the set under repetitive loading. He worked with greater stresses and obtained a greater rate of increase.

Hairline cracks became visible on the first few applications of working loads. They formed at intervals of from 6 to 8 in. along the beam. Examinations with a reading glass were made from time to time and the depth of the cracks marked. In some instances a crack could be traced 2 to 3 ins. deeper on one day than on the preceding, but usually the change in depth was small. Some cracks did not become visible till after a great number of applications had been made. None of them extended deeper than the neutral axis of the beam for repetitions of working loads. Fig. 3 gives definite data for three of the beams.

The diagrams for deformation in the plane of the steel also show a permanent set which is greatest for the first few applications

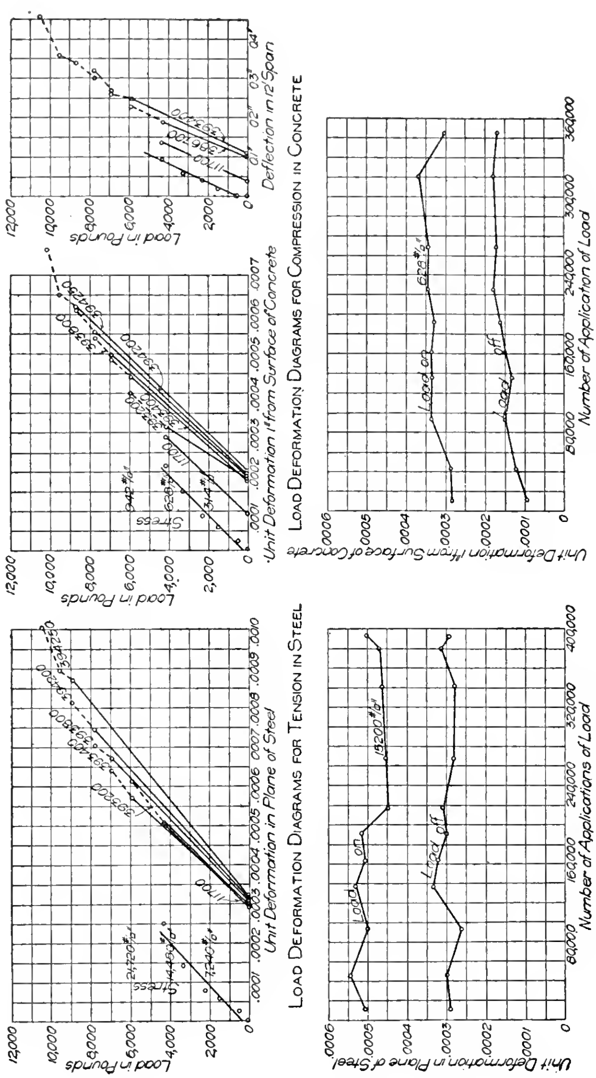

FIG. 7.—Results of Test on Beam No. 6.

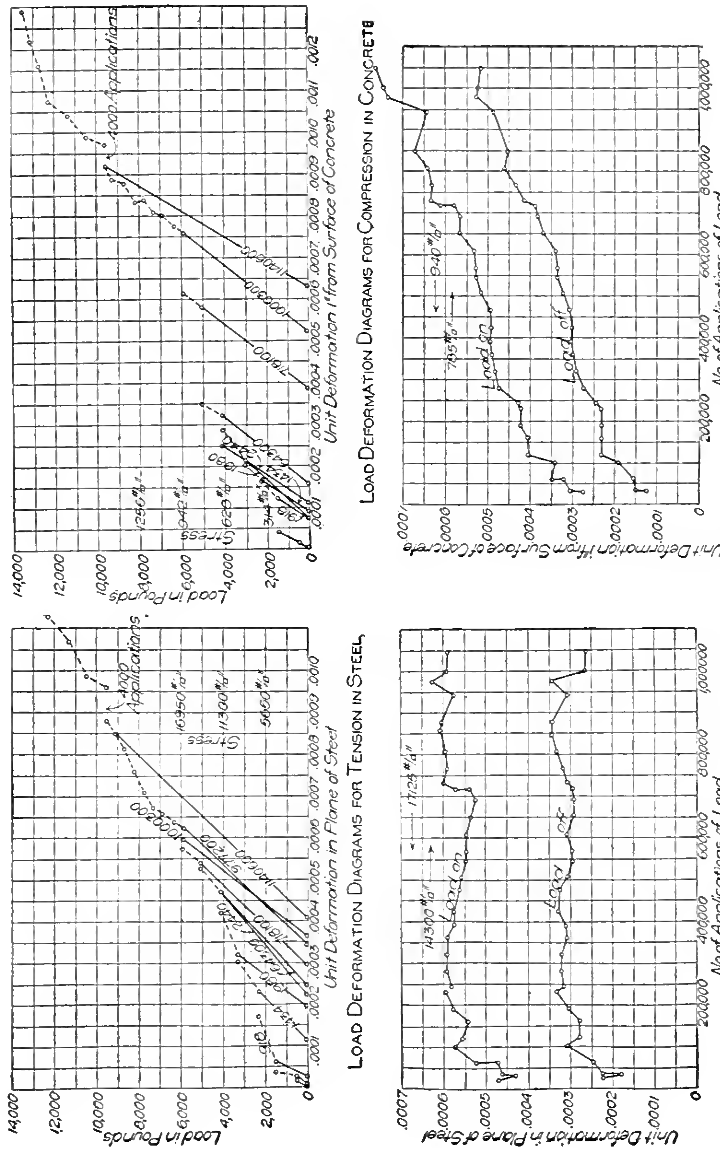

FIG. 8.—Results of Test on Beam No. 9.

of the load. This set did not increase after the first few thousand applications of the load in the case of three of the beams, viz., No. 6, reinforced with three plain bars $\frac{5}{8}$ in. square, No. 4, with three Corrugated bars $\frac{3}{4}$ in. square, and No. 9, with two Diamond bars $\frac{3}{4}$ in. in diameter. The set did increase throughout the test in case of Beams 2 and 8. For Beam 2, reinforced with four plain rods $\frac{1}{2}$ in. in diameter, the set after 290,000 repetitions was about twice as great as at 10,000, but it was then about the same as for the other beams subjected to a corresponding stress.

The beams tested to failure under continuous loading show about one-half as much set in the deformation in the plane of the steel for one application and release of a working load as the beams

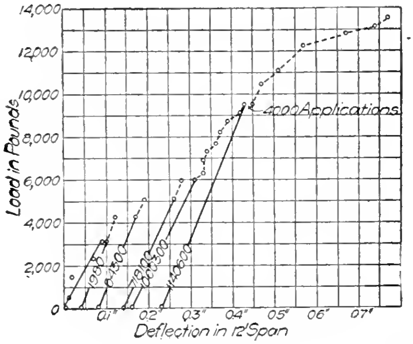

FIG. 9.—Load Deflection Diagram for Beam No. 9.

subjected to many thousand applications of a similar load. The set in the latter case was usually about one-half the total deformation at failure of the beams. This set may be due in part to a set or initial slip in the bond, but there were no indications of any material weakening of the bond when the beams were broken up for removal from the laboratory.

The load-deformation diagrams for the compression side of the beams show the same characteristics as were noted in case of the deflections. The effect on the set is greatest for the earlier loadings, but as with the deflections the set increases directly with the stress and the number of repetitions, as is shown by the inclination of the "Load On" and "Load Off" curves. The apparent exception, Beam 4, is explained by the accidental disturbance of one side

of the apparatus. If the readings for the opposite side only be used, the curve shows the same characteristics as the others. Table IV is derived from the slopes of lines drawn on the curves averaging the observed points.

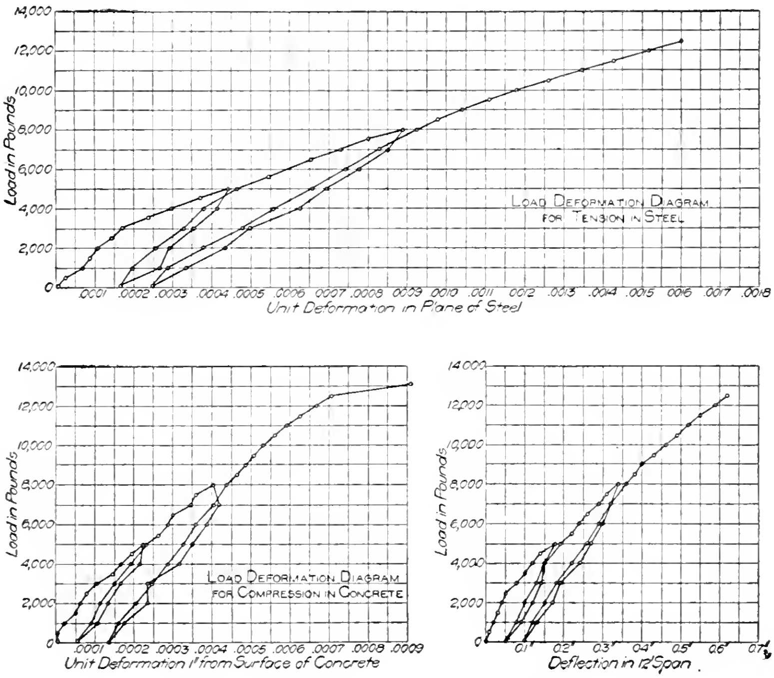

FIG. 10.—Results of Test on Beam No. 10.

TABLE IV.—RELATION OF REPEATED STRESS TO INCREASE IN SET.

Beam No.	Stress Repeated in the Concrete.	Increase in Set per 100,000 Repetitions.
6	628	0.000014
2	785	40
4	942	50
9	785	32
9	942	40

The above does not include the initial set from the first 10,000 applications, which was usually about one-half as great as the set after from 200,000 to 400,000. Beam 9 had at 1,000,000 repetitions about four times the set present at 10,000. The total amount

of the progressive set for this high stress, 940 lbs. per sq. in., was somewhat less than one-half the total deformation at failure of the beam.

Since the distance between the "Load On" and "Load Off" curves is very nearly constant for all the beams while the load remained the same, it is evident that the position of the neutral axis is not affected by repeated loading.

These tests, for which this paper must be considered a progress report, indicate:

1. That the ultimate strength of a reinforced concrete beam is not materially affected by one million repetitions of high working stresses.

2. That the maximum deflection is not affected.

3. That hairline cracks become visible for such loads at intervals of 6 to 8 ins. and grow deeper as the number of repetitions is increased, but that for one million repetitions no crack extended beyond the neutral axis.

4. That the bond between the steel and the concrete is not appreciably affected, as shown by the difficulty with which the steel was removed in breaking up the beams.

5. That the position of the neutral axis is not changed by repetitions of the load.

6. That the greater part of the set in the deformation in the plane of the steel occurs in the first few thousand applications of the load.

7. That the set in the deformation on the compressive side of the beam is also relatively large for the first few thousand repetitions, and that it increases with the stress applied and the number of repetitions.

TESTS OF BOND IN REINFORCED CONCRETE BEAMS.

By Morton O. Withey.

This paper is a report of tests made on the bond between concrete and plain round mild steel reinforcing bars. These tests were made in the Materials Testing Laboratory at the University of Wisconsin during the spring and summer of 1907.

The subject of bond between concrete and steel has led to many extensive series of tests. Most of these tests have been made by embedding a rod in the center of a cylindrical block of concrete and pulling it out in such a way that the concrete around the rod was compressed. In general, the results of such tests have neither given conclusive nor reliable data upon which working values for structural purposes can be based.

In order to approximate more closely the actual conditions in reinforced concrete beams, a different form of bond specimen was used in the tests herein described and the results compared with those obtained from the ordinary cylinder test. The larger portion of the specimens were made and tested as thesis work by Messrs. A. B. Carey, S. L. Clark and P. B. Johnson, of the class of 1907, to whom due acknowledgment is hereby made.

The sand used in these tests was a rather fine local bank sand. It weighed 103 lbs. per cu. ft., measured loose, contained 37.5 per cent. voids and 3 per cent. of fine dirt.

The limestone used was obtained from Kankakee, Ill. It would pass a 1-in. mesh, weighed 83 lbs. per cu. ft. and contained 48.5 per cent. voids. A local bank gravel was used in beams 19 and 20, particles less than 0.2 in. in diameter being screened out. It weighed 105 lbs. per cu. ft. and contained $35\frac{1}{2}$ per cent. voids.

The Atlas cement from Missouri took an initial set in 45 minutes, final set in 5 hours and 40 minutes and satisfactorily passed the pat tests. At 7 and 28 days the average strength of four neat briquettes was 529 and 659 lbs. per sq. in., respectively. The strength of the 1 : 3 mortar briquettes increased from 204 lbs. per sq. in. at 7 days to 309 lbs. per sq. in. at 28 days. Plain round mild

steel rods with an elastic limit of about 44,000 lbs. per sq. in., an ultimate strength of approximately 64,000 lbs. per sq. in. and a modulus of elasticity averaging about 30,000,000 lbs. per sq. in. were used as reinforcement in the test pieces. These rods were free from rust and loose scales.

The concrete for beams 1 to 49 was mixed by hand on the cement floor in the laboratory. The rest of the beams were mixed by machine. Materials were proportioned by volume. Quantities required were weighed on a platform scale. The weight of the cement was assumed to be 100 lbs. per cu. ft.

Three consistencies were used in beams 23 to 31, wet, medium and dry. The wet concrete was sloppy, could not be piled up on the mixing floor, and when placed in the molds showed an excess of water on top. The medium concrete when piled on the floor would flatten out, could be readily worked around the reinforcement without much tamping and was easily surfaced with a trowel. The dry concrete was very mealy, would stand piled up on the floor, required hard tamping and could not be readily surfaced with trowel. The medium consistency was used in all other beams.

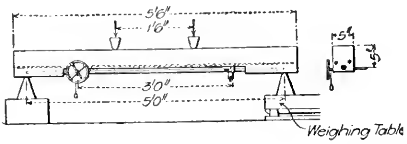

FIG. 1.

The form of specimen adopted for determining the bond in beams is shown in Fig. 1.* These beams were in general 5 ft. 6 in. long, 5 in. wide and 6 in. deep. They were so made that the central portion of the lower rod, whose bond was to be obtained, was exposed. The two $\frac{3}{8}$-in. round rods seen in the figure were placed above the exposed bar to distribute the tension cracks and prevent failure in the concrete before the lower rod slipped. Wooden forms were used in making beams 1 to 49, steel forms in the remainder.

In beams, the exposure of the lower rod was secured by covering it with damp sand before the concrete was placed. A notched piece of wood placed transversely with respect to the axis of the beam served to space the rod and provide a definite limit for the

* Acknowledgment is made to the *Engineering Record* for the cuts used in this paper.

TABLE I.—DATA ON SPECIMENS MADE.

Beam No.	Mix.	Consistency.	Size of Rod, ins.	Age.	Variable Studied.	Kind of Auxiliary Specimens.
1-2-3	1 : 2 : 4	medium	$\frac{5}{8}$	7 da.	time	compression
4-5-6	"	"	$\frac{5}{8}$	28 "	"	"
7-8-9	"	"	$\frac{5}{8}$	60 "	"	"
10-11-12	"	"	$\frac{5}{8}$	6 mos.	"	"
7-8-9	"	"	$\frac{5}{8}$	60 da.	mix	"
17-18	1 : 3 : 6	"	$\frac{5}{8}$	"	"	"
*19-20	1 : 2 : 4	"	$\frac{5}{8}$	"	"	"
21-22	1 : 2$\frac{1}{2}$	"	$\frac{5}{8}$	"	"	"
23-24-25	1 : 2 : 4	wet	$\frac{5}{8}$	"	consistency	
26-27-28	"	medium	$\frac{5}{8}$	"	"	
29-30-31	"	dry	$\frac{5}{8}$	"	"	
36-37	"	medium	$\frac{5}{8}$	"	method of test	bond
38-39-40	"	"	$\frac{3}{8}$	"	size of rods	"
41-42-43	"	"	$\frac{1}{2}$	"	"	"
44-45-46	"	"	$\frac{3}{4}$	"	"	"
47-48-49	"	"	1	"	"	"
54-55-56-57	"	"	$\frac{5}{8}$	28 da.	time and loading	compression
58-59-60-61	"	"	$\frac{5}{8}$	"	loading	"
62-63-64	"	"	$\frac{5}{8}$	29 da.	"	bond and compression

predetermined length of embedment. The embedment used for all beams with lower rod $\frac{5}{8}$-in. in diamter or larger was approximately 10 in., except in beams 62, 63 and 64, where it was approximately 15 in. The embedment of the lower $\frac{3}{8}$-in. rod in beams 38, 39 and 40 was nearly $7\frac{1}{2}$ in., and that of the $\frac{1}{2}$-in. rod in beams 41, 42 and 43 about $8\frac{1}{2}$ in. After each test, the actual length of embedment of the end at which slipping occurred was carefully measured. This quantity was then used in obtaining the results given in the tables.

Auxiliary compression specimens consisting of 4-in. cubes or cylinders 6 in. in diameter and 18 in. long were made with nearly every batch of concrete as indicated in Table I. The bond cylinders were also cast from the same batch of concrete as the corresponding beams. In all cases they were made with the rod vertical and an embedment of practically 12 in.

Beams 1 to 49 were kept in the molds for at least three days after mixing. They were then allowed to cure without being

* In 19 and 20 gravel was used in place of limestone.

TABLE II.—EFFECT OF TIME ON THE BOND OF 1:2:4 CONCRETE.

Beam No.	Maximum Load, lbs.	Age.	Stress in Steel, lbs. per sq. in.	Bond, lbs. per sq. in.	Compressive Strength of Concrete, lbs. per sq. in.
1	4,000	7 days	14,200	222	....
2	3,400	"	13,500	211	....
*3					
Average	......	"	......	216	1,400
4	6,800	28 days	23,000	305	2,030
5	4,400	"	19,200	312	
6	4,600	"	12,900	207	
54	4,500	"	14,500	228	1,690
55	4,500	"	15,900	248	
56	4,300	"	14,100	222	
57	5,000	"	16,300	247	
Average	......	"	......	253	1,860
7	4,800	60 days	18,500	362	....
8	4,200	"	16,900	264	....
9	4,000	"	11,100	201	....
Average	......	"	......	276	1,790
10	4,200	6 mos.	17,900	280	....
11	5,200	"	21,300	350	....
12	4,800	"	19,800	318	....
Average	......	"	......	316	1,630

dampened until tested. Beams 50 to 61 were kept in the forms two days and sprinkled every day for a week after mixing. Beams 62, 63 and 64 were kept in the forms three days, and sprinkled twice a day until tested. This continued wetting may have weakened their bond strength. Table I gives the general data on the 56 beams which were made.

All beams were tested over a 5-ft. span. All beams except 62, 63 and 64 were loaded as shown in Fig. 1. Fig. 4 (*b*) indicates how beams 62, 63 and 64 were loaded and also shows the different shape adopted for beams 58 to 61. Loads were applied in increments of 200 or 300 lbs. and readings of the deformation in the rod taken on the extensometer.

The deformation of the rod was measured by a dial extensometer reading to 0.0001 of an inch, shown in Fig. 2. This extensometer was described in a paper presented before this Society last year by Professor H. F. Moore.† It is only necessary to say in

* Beam No. 3 was accidentally destroyed.

† Volume VII, 1907, page 607.

TABLE III.—COMPARISON OF THE BOND OF VARIOUS MIXES. AGE, 60 DAYS.

Beam No.	Mix.	Maximum Load, lbs.	Stress in Steel, lbs. per sq. in.	Bond, lbs. per sq. in.	Compressive Strength of Concrete, lbs. per sq. in.
7	1:2:4	4,800	18,500	362	
8	"	4,200	16,900	264	
9	"	4,000	11,100	201	
Average	"	......	......	276	1,790
17	1:3:6	3,200	11,600	181	
18	"	4,400	18,100	251	
Average	"	......	......	216	830
19	1:2:4 g	5,200	19,900	282	
20	"	4,800	17,200	269	
Average	"	......	......	275	2,200
21	1:2½	3,600	16,600	235	
22	"	3,800	18,200	300	
Average	"	......	......	267	1,600

regard to it that an average deformation of both sides of the rod was secured from a single reading of the dial. The readings are considered sufficiently accurate for this class of work. As the rod in the beam was horizontal instead of vertical, the weight, *W*, hung down perpendicular to the plane of the paper and not as shown in Fig. 2. Slipping of the rod was immediately indicated by a backward movement of the pointer on the dial. The dropping of small particles of sand and concrete from the surface of the rod where it entered the beam made evident the end which slipped.

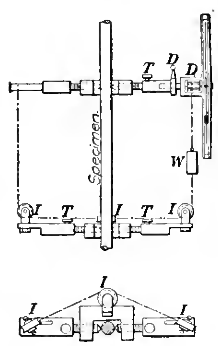

FIG. 2.

The rods embedded in cylinders were pulled out by a universal 50,000-lb. testing machine. The pulling head was raised so that the projecting part of the rod could be run up through it and gripped by the wedges in the upper head. If the top of the cylinder was uneven, blotting paper was inserted between it and the pulling head to equalize the pressure. No allowance was made for the area of the hole in the pulling head, 8 sq. ins., in computing the stresses in Table VII.

TABLE IV.—EFFECT OF DIFFERENT CONSISTENCIES ON THE BOND OF 1:2:4 CONCRETE. AGE OF SPECIMENS, 60 DAYS.

Beam No.	Consistency.	Maximum Load, lbs.	Stress in Steel, lbs. per sq. in.	Bond, lbs. per sq. in.
23	wet	3,200	11,000	180
24	"	4,800	22,100	346
25	"	4,000	16,500	224
Average	"	......	......	250
26	medium	3,200	11,800	171
27	"	3,600	18,500	321
28	"	3,500	13,600	212
Average	"	......	......	235
29	dry	4,800	19,300	287
30	"	3,600	20,100	314
31	"	3,600	13,600	224
Average	"	......	......	275

To calculate the bond in a beam from the deformation of the rod, the following formula was used:

$$B = r\,d\,E \div 8\,L\,l$$

where B = bond in pounds per square inch, r = dial reading plus zero correction, d = diameter of rod in inches, E = 30,000,000 lbs.

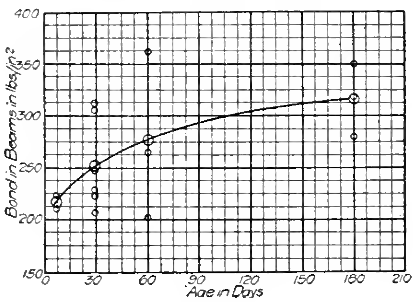

FIG. 3.

per sq. in., L = gauge length of extensometer, and l = length of embedment of rod.

In Table II are given the results of beam tests to determine the effects of time upon the bond. Although there was a considerable variation from the mean at some of the ages, the average

TABLE V.—EFFECT OF SIZE OF ROD ON THE BOND OF 1:2:4 CONCRETE. AGE, 60 DAYS.

Beam No.	Size of Rod.	Maximum Load, lbs.	Stress in Steel, lbs. per sq. in.	Bond, lbs. per sq. in.
38	$\frac{3}{8}$ in.	3,200	27,600	345
39	"	3,400	23,800	298
40	"	2,400	16,700	190
Average	"	......	......	278
41	$\frac{1}{2}$ in.	4,400	26,400	361
42	"	3,600	21,200	312
43	"	2,800	12,300	186
Average	"	......	......	286
7	$\frac{5}{8}$ in.	4,800	18,500	362
8	"	4,200	16,900	264
9	"	4,000	11,100	201
Average	"	......	......	276
44	$\frac{3}{4}$ in.	5,000	12,400	207
45	"	4,400	13,900	289
46	"	6,200	15,700	295
Average	"	......	......	264
47	1 in.	3,400	5,850	136
48	"	4,000	7,650	174
49	"	4,000	7,900	180
Average	"	......	.....	163

bond increased with the age of the specimen. This is well shown by the time-bond curve in Fig. 3, drawn through these plotted averages.

Table III contains the results of bond tests made on different mixes. The bond for the 1:3:6 mix was decidedly lower than the others. It will also be noted that the compressive strength of this mix was only about half that of the other mixes. There was very little difference in the average bond of 1:2:4 limestone, 1:2:4 gravel and 1:2½ mortar.

The data for tests upon wet, medium and dry consistencies in Table IV indicate that the amount of water used in these tests had no decided effect on the bond when the specimens were 60 days old.

From Table V it appears that the size of the rod up to 1-in. diameter has but little influence on the bond. The specimens with the 1-in. rod showed a marked decrease in bond. More bond tests

TABLE VI.—MISCELLANEOUS TESTS ON THE BOND OF 1:2:4 CONCRETE.

Beam No.	Maximum Load, lbs.	Stress in Steel, lbs. per sq. in.	Bond, lbs. per sq. in.	Compressive Strength of Concrete, lbs. per sq. in.
54	4,500	14,500	228	......
55	4,500	15,900	248	......
56	4,300	14,100	222	......
57	5,000	16,300	247	......
Average	......	......	236	1,690
58	3,400	8,800	129	......
59	3,300	9,500	141	......
60	4,100	12,600	187	......
61	3,900	13,700	204	......
Average	......	......	165	1,070
62	6,850	19,000	197	......
63	6,800	20,300	215	......
64	7,000	22,100	236	......
Average	......	......	216	2,315

with this size of bar are necessary before this decrease can be satisfactorily accounted for.

Beams 54 to 61 were made to determine the effect on bond of embedding the rod in stressed and unstressed concrete. Beams 54 to 57 were made and loaded, as shown in Fig. 1. Fig. 4 (*b*)

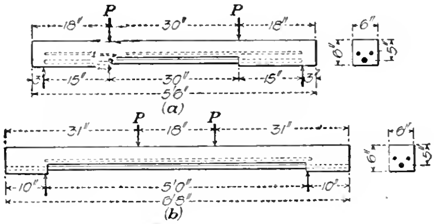

FIG. 4.

represents beams 58 to 61. The results apparently indicate that the bond of concrete subjected to a tensile stress is considerably greater than that of concrete which is practically unstressed. It is possible that bending increased the resistance against slipping in the first four of these beams. It will be observed also that compressive strength of the concrete for beams 54 to 57 was considerably higher than that tested from beams 58 to 61.

Beams 62, 63 and 64 were designed to determine the effect of a change from the standard loading, shown in Fig. 1. They were made and loaded as shown in Fig. 4 (*a*). The average bond of these beams did not differ materially from that obtained from beams 54 to 57. The small decrease in bond developed in 62, 63 and 64 might have been due to the fact that they were sprinkled more frequently while curing than the other beams. This might have decreased the shrinkage of the concrete and thereby lessened the bond.

If the bond stress be computed in the ordinary way from the

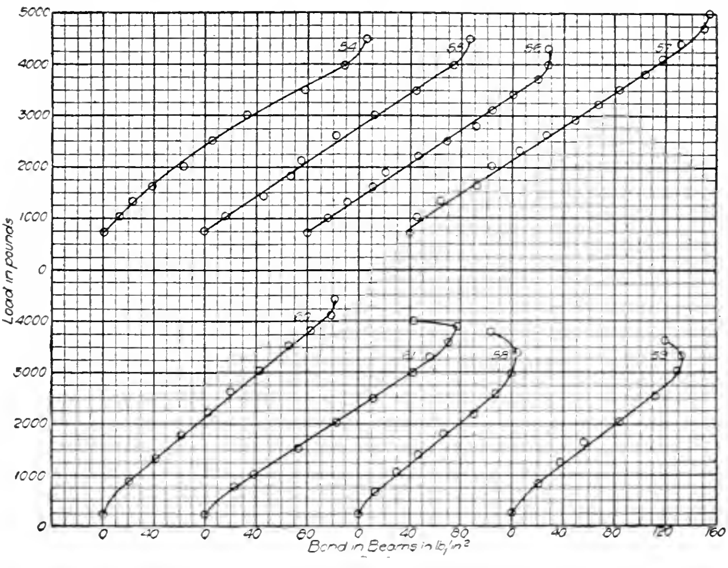

FIG. 5.

average vertical shear of beams 62, 63 and 64, a value of 214 lbs. per sq. in. will be obtained. In making this computation the arm of the resisting couple was taken as 0.82 times the effective depth. The distance from the top of the beam to the $\frac{3}{8}$-in. rods was $3\frac{3}{4}$ ins. This quantity checks very closely the average value obtained from the deformation of the lower rod, 216 lbs. per sq. in.

Fig. 5 shows load-bond curves for beams 54 to 61. It will be seen that all the curves show a reversal of curvature at the load at which the maximum bond was developed.

A comparison between the bond developed in beams and that obtained in bond cylinders made at the same time is afforded in Table VII. Column 5, headed "Ratio, Bond in Cylinder to Bond in Beam," shows that the cylinder tests gave bond stresses from 1.42 to 2.99 times as large as those obtained from the beam tests. Fig. 6 is a curve plotted from the values in the last two columns of Table VII. It indicates that the ratio of bond in cylinder to bond in beam increases with the compressive stress applied to the top of the bond cylinder. The upper part of the curve was drawn

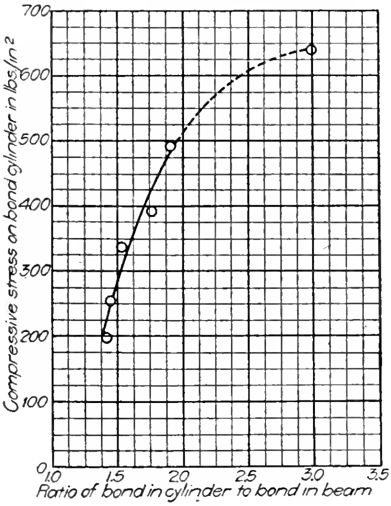

FIG. 6.

a dotted line as more tests of 1-in. bars are needed to fix its position definitely.

CONCLUSIONS.

1. The bond of 1 : 2 : 4 concrete to embedded steel increases with age at least up to six months. About 80 per cent. of its six months bond strength is developed at 28 days.

2. Owing to the variation in results of the one-month tests, and the wide difference between laboratory experiments and practical working conditions, it does not seem as though the maximum bond of 1 : 2 : 4 concrete should be assumed greater than 200 or 250 lbs. per sq. in. in designing.

TABLE VII.—COMPARISON OF BEAM AND CYLINDER TESTS ON BOND.

Beam No.	Size of Rod, ins.	Average Bond in Beam, lbs. per sq. in.	Average Bond in Cylinders, lbs. per sq. in.	Ratio, Bond in Cylinder to Bond in Beam.	Compressive Stress on Top of Bond Cylinder, lbs. per sq. in.
38-39-40	$\frac{3}{8}$	278	394	1.42	197
41-42-43	$\frac{1}{2}$	286	455	1.54	317
62-63-64	$\frac{5}{8}$	216	311	1.44	252
36-37	$\frac{5}{8}$	266	467	1.76	395
44-45-46	$\frac{3}{4}$	264	502	1.90	495
47-48-49	1	163	487	2.99	620

3. The method of making bond tests by pulling a rod from a concrete cylinder, as previously described, gives results which are of neither quantitative nor qualitative value. The results obtained are dependent largely upon compressive stress acting on the head of the cylinder.

4. The beam test for bond on the other hand approaches closely the actual conditions to which the bar and surrounding concrete are most often subjected, and gives values which are at least of qualitative value. Furthermore, values obtained by this method are in accord with the bond theory usually accepted as a basis in design.

These tests have been continued during the past year. The bond of both plain and deformed bars, when subjected to static and repeated loadings, has been investigated, and the results of all of these tests will appear during the year in a University of Wisconsin bulletin.

CEMENT AND CONCRETE WORK OF UNITED STATES RECLAMATION SERVICE, WITH NOTES ON DISINTEGRATION OF CONCRETE BY ACTION OF ALKALI WATER.

By J. Y. Jewett.

It is not the purpose of this paper to give any extended account of the construction work of the United States Reclamation Service, in which cement and concrete are being used, as such description would hardly come within the scope of the field which this Society aims to cover. It was thought, however, that a brief statement regarding the work being done in this line by the Reclamation Service, which is one of the newer branches of the government to carry on construction work on a large scale, might be of sufficient interest to warrant giving it space in the program, the subject being taken up more especially from the point of view of the materials used and tests of the same, rather than as relating to general features of construction.

The interest attaching to the work of the Reclamation Service, as relating to the use of cement, lies largely in the wide scope and variety of the work carried on rather than in the use of cement in large quantities, for while considerable quantities are being used, and while the Service has a cement mill of its own at one point (where a large masonry dam is being built in a region remote from railway facilities), still, on the other hand, such structures as earth and rock-fill dams are being employed at various points, so that the total amount of cement used is not as large by comparison as that called for by other works of lesser magnitude as to extent and cost of the work as a whole.

With regard to the scope and variety of the field covered, however, it may be noted that construction work involving the use of cement and concrete is being carried on at twenty-five projects, located in fifteen states and territories, including in extent of area the whole western half of our country and having a range in the matter of temperature and climate from 120° in the shade at Yuma, Ari-

zona, to 40° below zero in northern Montana. Among the structures included may be noted three large masonry dams, one of which, when completed, will be the highest in the world, and in addition a large variety of other structures, mainly of concrete, both plain and reinforced, including diversion dams, tunnel linings, canal and ditch linings, pumping stations, gate houses, pressure pipes, flumes, conduits, culverts, bridges, etc.

This wide variety in the structures and their locations involves also the use of an interesting variety of materials, both as stone masonry and concrete aggregates, depending on the materials available for this purpose at the sites of the different projects. While in some cases satisfactory materials have been found readily available, in other cases it has been a question between using the materials at hand or of procuring a more satisfactory supply from a distance at an added expense, and in some cases calling for a comparative investigation, both on the ground and in the laboratory of the value of the materials in question. Thus, in this connection, the value of limestone screenings and of crusher screenings in general, for use in the place of sand, has been investigated, as well as the comparative value of various grades of sand; and in the case of two of the large dams already mentioned, in the absence of a supply of natural sand, the sand required is obtained by crushing and grinding the rock from which the dam is being constructed, this being in one case a granite and in the other a magnesian limestone. The question of the use of sand-cement on certain projects remote from railway facilities has also been investigated, and an extensive series of tests has been made on this material.

As already stated, no detailed account of the various works under construction can be given here, but from what has been said it will be readily seen that the conditions under which the work is carried on call for a wide adaptation to varying conditions and circumstances on the part of the engineers in charge of their design and construction, especially in remote regions, where to the ordinary difficulties of construction work are added difficulty of access and of procuring proper materials.

Coming to the matter of tests of the cement used, it may be stated that interest here also attaches, not so much to any large amounts handled or to any unusual methods adopted as to the broad field covered by the work. The main laboratory for carry-

ing on these tests is now located at Chicago, with a smaller laboratory at Berkeley, Cal., for the Pacific coast states, and the cement used has been, or is being, obtained from plants located in the states of Illinois, Kansas, South Dakota, Colorado, Utah and California. Owing to conditions in the Western states it is of course necessary to transport the material over much longer distances than in the Eastern states and in the case of the Reclamation Service cement has been shipped at times from Chicago as far west as Boisé, Idaho —a distance of over 1,500 miles.

The specifications under which the cement is purchased are based in their main features on the standard specifications of this Society, although these have not been formally adopted by name, and the laboratory tests are in general conducted in accordance with the methods prescribed by those specifications, thus putting the Reclamation Service in line with the purpose of this Society to bring about the unifying and standardizing of specifications and methods of testing, with which movement the writer is heartily in accord and toward which he has used his influence as far as possible.

As to the question of making tests for acceptance at the plant or after arrival on the work, it may be stated that the Reclamation Service has found it desirable to adopt the former as its general policy. This policy is made necessary principally for the reason that most of the cement used by the Service is purchased f. o. b. at the factory and shipped on government bills of lading, which virtually renders the acceptance of the material at the factory necessary; and even if this were not the case this course would still be desirable on account of the long freight haul already mentioned, to which is added, in many cases, a long wagon haul after arrival at destination, this being in the case of one of the large dams under construction, a distance of about fifty miles. Under these conditions it will readily be seen that in case acceptance tests were made after arrival at destination the possible rejection of unsatisfactory material would be a matter of very considerable expense and delay in various ways.

The method adopted of conducting the tests previous to shipment is, in general, to make bin tests at the various plants from which cement is being obtained, samples being taken from the bins in such a manner as to obtain representative samples of the

lot under test. These samples are then sent to the laboratory where the tests are to be made, the bins meanwhile being held under seal until passed upon as to acceptance or rejection. In case of acceptance the required shipments are made under the direction of a representative of the Service, who inspects the loading, seals the cars, and bills them out on government bills of lading, as already mentioned. With regard to the method of sampling the bins, and the number of samples to be taken, etc., no hard and fast rule can be laid down. The method adopted varies in accordance with conditions existing at the different plants as to arrangement of bins, etc., the purpose in each case being to obtain samples that will be fully representative of the lot under test.

After reaching the laboratory the samples are tested in accordance with the standard methods of this Society, as already stated, tests being made on individual samples and the results averaged rather than on a composite sample made up from a mixture of individual samples. The writer believes that this method, especially in the case of the work in question, gives better information as to the character of the material than would be the case if the other method were used.

In addition to the standard tests for acceptance occasional sets of long time tests are made for various periods, from one day up to ten years, on samples taken in such a way as to be generally representative of the various brands tested. With reference to these tests it may also be noted that a special point is made of obtaining a set of such tests on any lot which may be rejected for any reason in order to see how the results thus obtained compare with the regular tests on the same brand for these long-time periods. Occasional chemical analyses are also made on each brand, more for the purpose of having a record of the chemical composition of the material than for purposes of acceptance or rejection, these analyses being in addition to the analyses for sulphuric anhydride regularly made on each lot tested.

With reference to the question of the acceptance or rejection of the material tested, among the various problems which come up may be mentioned those relating to the value to be given to the results obtained from the hot water or steam test for soundness; to the amount of sulphuric anhydride allowable, and as to whether the present limit of 1.75 per cent. could be exceeded in certain cases

without injury; and to the value to be placed on the specific gravity test, which matter has been under discussion by this Society during the last year or two.

With regard to the hot water test for soundness, which has been such a fruitful subject for discussion in times past, our general practice has been, as a matter of safety, to reject any lot showing serious unsoundness in this test, and it may be noted that long time tests on lots rejected for unsoundness show results justifying this action. On the other hand, however, with relation to the question of sulphuric anhydride it may be noted that long time tests on a lot rejected as containing 3.20 per cent. of SO_3 do not show any unfavorable results up to the two-year period as compared with the regular tests on the same brand for the same period. This instance is not cited as an argument for raising the limit for SO_3 but merely suggests the possibility that a full investigation of the subject might show that for all ordinary purposes the limit could be placed at a slightly higher figure than at present without injury.

DISINTEGRATION OF CONCRETE BY ALKALI WATER.

Coming now to some of the difficulties met in construction work by the engineers of the Reclamation Service, there may be mentioned a matter which has recently been brought to the writer's notice, relating to the disintegration of concrete structures by the action of alkali water, and which it is thought may be a new and interesting phase of construction work to most members of the Society.

As is well known, there are numerous deposits of so-called "alkali" in the arid regions of the west and in many places the running waters of these regions are strongly impregnated with these alkaline salts in solution. This general term "alkali" may include a variety of substances in larger or smaller amounts, of which the salts of sodium and potassium are among the most common, although salts of calcium and magnesium are also included in the general term.

As the use of cement and concrete to any large extent in these regions is a comparatively recent matter, any effect that this alkali water might have on structures with which it comes in contact has been largely a matter of conjecture, if it has been considered at all.

Recently, however, the matter has been brought forcibly to the attention of the engineers of the Reclamation Service as well as to engineers in other lines of work in some of these Western states by the disintegration of concrete structures with which water of this character has come in contact.

This action has been particularly severe on the Sun River project, located in the State of Montana, near the City of Great Falls, at which point trouble in this line has been experienced on municipal work, as referred to later on. It has also been noticed on certain structures on the Garland Canal on one of the Wyoming projects. However, as some, though not all, of the other projects of the Service are located in districts where alkali exists it is apparent that its destructive effect is dependent on its composition in a given locality, as will appear during the further discussion of the matter.

On the Sun River project above mentioned its effect has been seen in the disintegration of a number of small structures, such as pipe culverts, which are partially submerged in the small streams carrying the drainage of the surrounding country. The effect of this water in causing the disintegration of the concrete is noticed, both in the softening of the material where constantly submerged for a period of three or four months, and also more particularly in the breaking up of the structure at the line where the water rises and falls, or, in other words, where there is a submersion followed by a drying out, followed by another submersion, etc. This process brings about a breaking up of the structure, similar in appearance to that of the effect of freezing, only much more severe. It is described by the engineer in charge of this work as follows:

> On examination of some of the concrete culverts which have been in place in alkali water for three or four months, the part under water is much softer than the part above water, but the greatest effect of the alkali seems to be at the water surface. Have examined pieces of these culverts taken from below water level and found, by microscopic examination, minute crystals which disintegrate on being exposed to the air and become a fine powder. This process of crystallization has an enormous expansive force, the same as freezing, and this undoubtedly has an enormous disintegrating effect. One culvert which had been in alkali water several months had no strength at all and was like so much sand and mud at the water level.

Of course, in this case, as in any other case of concrete failure, the question of poor workmanship is one of the first to be considered and it should be noted that the work in question was carried on by force account under direct supervision of employees of the Service and not by contract, and was done in the usual thorough and substantial manner in which all of the construction work of the Service is carried on. After taking all the evidence into account there seemed to be no other conclusion but that the action of the alkali water was directly responsible for the results noticed.

In studying the cause of this disintegration it seemed that a chemical analysis of the water producing these results would be the first step of importance to be taken. The results obtained from an analysis of this water are as follows:

	Milligrams per Liter.
Calcium sulphate ($Ca\,SO_4$)	1,690
Magnesium sulphate ($Mg\,SO_4$)	6,870
Basic magnesium carbonate ($Mg\,H_2\,CO_2$)	305
Magnesium chloride ($Mg\,Cl_2$)	192
Potassium chloride ($K\,Cl$)	20
Total solids	9,077
Weight after ignition	8,855
Loss on ignition	222

From this analysis it will be seen that the principal mineral ingredient of this water is magnesium sulphate and this leads to the conclusion that the effect of this water in causing disintegration of concrete is similar in its action to that of sea water in which magnesium sulphate is stated to be the principal substance acting to bring about the disintegration of concrete with which it comes in contact.

This question of the action of sea water on cement has been the subject of much discussion and investigation among European cement authorities, and, without going into details as to the chemical actions involved, it may be stated as the generally accepted conclusion of these authorities that magnesium sulphate in the sea water acts on the calcium hydrate of the cement, forming calcium sulphate, and further, that this calcium sulphate combines with the alumina of the cement, forming a calcium sulpho-aluminate. As to the injurious action of these compounds, Le Chatelier, in a

report on "The Behaviour of Cement in Sea-water," presented at the last Congress of the International Association for Testing Materials, held at Brussels in 1906, makes the following statement:

> When the calcium sulphate found in natural waters or formed by the interaction of magnesium sulphate and the calcium compounds of cement reacts with calcium aluminate, it produces a calcium sulpho-aluminate whose crystallization gives rise to swelling and cracking in the material. The action resembles that consequent upon the hydration of quicklime, but is much slower.

Again, Dr. William Michaelis of Germany, in a paper published in 1896 on "The Influence of Sea Water on Hydraulic Mortars," says:

> The simple formation of sulphate of lime with two equivalents of water implies a considerable increase of volume and in itself is sufficient to destroy the cohesion attained during the absorption of water. But together with this there occurs the formation of sulphates and aluminates of lime which implies an immense increase of volume and a corresponding total destruction of all cohesion.

Tending, however, to counteract this destructive effect, which, if it went on continuously, would produce the complete disintegration of any structure exposed to its action, certain salts, such as the magnesium hydrate formed by the chemical action above mentioned, have, it is said, a tendency to fill the pores of the concrete, so that in this way, and in other ways, the material may become gradually more impervious, thus checking the disintegrating effect to which it is subjected.

As to any difference in the susceptibility of various cements to this destructive action, it is generally conceded that cements rich in alumina are more readily acted upon in this way, as stated in the following additional quotation from Michaelis, which also refers to the method above mentioned by which this effect is counteracted:

> Portland cements, rich in alumina, are especially susceptible to decomposition by a solution of sulphate of calcium, and most maritime structures built with these binding media have been saved from complete and early ruin merely through such favoring circumstances as impermeability, absorption of carbonic acid, incrustation, closure of pores, etc.

While, as stated above, it is apparent that the injurious effect of water containing magnesium sulphate in the case in question is similar to that of sea water, as also stated, these alkali deposits vary considerably in composition in different localities, sodium salts, such as the sulphates and carbonates, being very common ingredients. It is probable, however, that sulphates in any form are injurious, as the following further quotation from the above mentioned paper by Michaelis goes to show:

> The injurious influence of sea water should no longer be attributed to the salts of magnesia since such injury results only from the sulphuric acid or from the soluble sulphates in the sea water. Sulphate of magnesia, it is true, is the specific sulphate acting in sea water, but sulphate of calcium, sulphate of any alkali—in short, every sulphate soluble in water—has precisely the same character of destructive influence, although not the same amount of energy.

On the other hand, however, it is probable that such salts as the carbonates and chlorides are not injurious and in this connection it may be of interest to note that before the above-mentioned chemical analysis of the water in question was obtained, acting on a suggestion that this alkali consisted largely of sodium carbonate, a set of comparative tests was made which showed a marked increase in tensile strength for briquettes immersed in a saturated solution of sodium carbonate over those immersed in the regular tanks in the laboratory.

In confirmation of the above it may be noted that Bulletin No. 69 on "The Effect of Alkali on Portland Cement," issued by the Experiment Station of the Montana Agricultural College, Bozeman, Mont., giving the results of an investigation of the destruction of sewers at Great Falls, Mont., by the action of alkali, shows in its reports of analyses of this alkali that its principal ingredients are calcium, sodium, and magnesium sulphates. This bulletin also states, that in addition to its effect on the concrete sewers and on the cement joints of brick sewers at this place, the bricks themselves are attacked by the alkali, and that the sandstone used in the foundations of buildings there is also being attacked and disintegrated by the same agent. It may also be of interest to note that this bulletin, besides referring to this action as compared with that of sea water, refers also to the possibility of this effect being

similar to that of ' zeolitic" action, as described by Dr. E. W. Hilgard in his work on soils.

To take up briefly the question as to the best method of preventing this destructive action in cases where alkali exists in injurious form, the main point is, of course, to produce an impervious concrete. As to the best method of accomplishing this, however, there is, as is well known, a wide variety of opinion. For instance, among the different authorities on the use of cement in sea water there are various opinions offered as to the best method of counteracting its destructive effect, such as the use of a richer and denser mixture than would ordinarily be used in making the concrete; the admixture of siliceous material in the form of puzzuolana or trass, which it is claimed will unite with the uncombined lime, which would otherwise be acted on by the salts of the sea water; the use of an iron-ore cement in which alumina is largely replaced with iron; the use of a cement with a lower lime content, etc., in addition to which may be mentioned the numerous methods of waterproofing which have been brought forward in recent years, of which some take the form of protective coatings applied to the surface, and others take the form of material to be incorporated in the body of the concrete during the process of mixing. The limits of this paper, however, will not admit of discussion of the relative merits of these various methods of rendering concrete impervious, as this subject furnishes a broad field for discussion in itself and in this connection the report of the Committee on Waterproofing of this Society at the present meeting will be looked forward to with considerable interest by the writer.

In connection with this matter of preventive measures to be used, the question of the materials used as concrete aggregates, and especially that of the sand used, is also of importance as having a possible bearing on the matter.

In the case of the sand used on the Reclamation Service project above mentioned, the engineers reported that there were evidences that this sand, which was the only supply readily available, contained alkali or some substance making it chemically active and assisting in the disintegration of the concrete. A chemical analysis of this sand was therefore obtained and resulted as follows:

		Per Cent.
Silica ($Si\,O_2$)		67.03
Alumina ($Al_2\,O_3$)		6.85
Ferric oxide ($Fe_2\,O_3$)		2.50
Manganese oxide ($Mn\,O$)		.02
Lime ($Ca\,O$)		10.72
Magnesia ($Mg\,O$)		1.72
Sulphuric anhydride (SO_3)		Trace.
Alkalies	Soda ($Na_2\,O$)	1.74
	Potassa ($K_2\,O$)	1.51
Water at 100° C.		.14
Ignition loss		8.16
Total		100.39

This sand gives excellent results from physical tests but since, as will be seen from the above analysis, the alkali content is about 3 per cent. which would correspond to about 15 to 20 per cent. of feldspathic material and since it also contains a considerable percentage of limestone, it is altogether possible that this sand would be more readily acted on by the influences tending to disintegrate the concrete than a sand of a more highly siliceous nature.

In conclusion, it may be observed that this whole question serves as an illustration of the inter-relation of chemistry and engineering in much of the industrial work of the present day, and as the writer approaches the subject more especially from the engineering side he would be pleased, in the discussion of the paper, to have the opinion of the chemists present as to the questions involved.

As was stated at the beginning, this subject is a new one; but is one which is likely to become of growing interest to certain regions of our Western states and is a subject calling for further and more extended investigation, which will undoubtedly be carried on by those interested in the matter.

In the meantime, however, the following suggestions are offered as practical measures to be taken by those who are engaged in construction work or who are considering the taking up of such work in these sections of the country.

1. An investigation of the character and composition of the "alkali" in any district where construction work is contemplated.

2. A study of the materials available for use as concrete aggregates, and involving the question, in case such are not alto-

gether satisfactory, of obtaining a more satisfactory supply from a distance.

3. Consideration of the best methods to be employed in any given case for producing an impervious concrete.

4. The above assumes that the cement to be used is of a reliable brand and of such quality as will pass the standard tests of this Society, as the use of any special cement, such as the "iron-ore" cement above mentioned, is probably out of question for these regions at the present time.

It is not the purpose of this paper to attempt to discourage in any way the rapidly growing use of cement in its manifold forms of application in these regions but rather, with the belief that "forewarned is forearmed," by calling attention to the danger which exists, to provide for the taking of steps to avert the same.

APPENDIX.

Herewith is submitted copy of a letter received by the writer from Mr. F. W. Brown, Superintendent of the plant of the Portland Cement Company, Portland, Col., containing a discussion of the subject in question:

Replying to your favor of June 6, I can only give you general ideas based on what I have observed in the use of cement in connection with "alkali" waters. This matter has not been gone into thoroughly in an experimental way and there is much to be learned. It is perfectly safe to say that in the ordinary use of cement concrete the work is so carelessly done that there is always danger of the work being seriously injured from the infiltration of "alkali" solutions. The alkali salts are contained in small percentages in many of the western clays, and are dissolved out by seepage water, which is usually strongly impregnated, varying from 50 grains per gallon to over 1,000 grains per gallon. These salts vary in character according to the nature of the clay, the more common being sulphate of soda and sulphate of magnesia. There are many sulphates and carbonates present in small amounts, but these two are the only ones that have thus far been demonstrated to be injurious in practice. The sulphate of lime is injurious if the percentage is carried too high in the concrete, but this salt is so sparingly soluble that water containing it can seldom become so concentrated as to injure the concrete work, although it is possible that it might do so.

The basic cause of the injurious action is supposed to reside in the sulphuric acid, but whether or not this is the case is open to question, as

the sulphates of magnesia and soda appear to be much more injurious than the sulphate of lime for a given amount of sulphuric acid. The evidence, so far as I have observed the practical condition, would indicate that it is the nature of the salt itself that causes the injury, while the percentage of salt present is readily indicated by a determination of the sulphuric acid. It is certain that the sulphates are far more injurious than the carbonates, or chlorides, but I attribute this more to the water of crystallization than to any other feature of the salts, since the sulphates alone carry very large quantities of water of crystallization, and the quantity that they carry is easily affected by their conditions, whether in solution or as dry salts, or whether acted upon by heat or atmospheric agencies, and I think in this peculiar characteristic is to be found the cause of their disastrous action.

In the case of sulphate of lime—so far as my experiments have gone—the amorphous state is the most injurious, while the uncalcined rock is very much less injurious when added to cement in large quantities than are the calcined products of the same rock. This is some evidence of the cause of injury.

There is another peculiarity of the sulphates which is believed to contribute to their disintegration of concrete; the sulphates very readily form double salts, that is, a simple sulphate will substitute a portion of its magnesia or soda for lime, iron or alumina, thus removing some constituent of the cement from the crystalline body, and substituting in its place a soluble alkali. These salts have been termed sulpho-aluminates, and it certainly is possible to obtain such a salt from the lixiviation of the mortar made from a mixture of sulphates and cement, and it certainly is true that where such cement mortar would be subjected to atmospheric evaporation of its water such crystals would be formed and be found in the mortar, but I do not consider that it is finally settled that this is the original action or the original cause, as it may be more in the nature of a result.

But the fact remains that when cement concrete becomes a depository of sulphates from infiltrating waters which are evaporated in any way from the concrete so as to leave the salts behind, the percentage of sulphate in the concrete will continue to increase until it destroys the concrete.

There are only two ways that I can see to avoid this result in soils that contain such water: One is to furnish a natural drainage which will cut off the water from permeating the concrete; and the other is to so prepare the concrete that it is impermeable to water. Danger is not to be anticipated where the concrete is continuously above or below the high and low water marks of such water in the soil. I have seen the concrete in an embankment where the upper portion of the apron was above high water mark of the alkali water and the lower portion of the apron was below the low water mark, and was standing in alkali water which contained over 1,000 grains to the gallon of magnesia sulphate. This concrete had been in this condition for some three years, the upper portion

as well as the submerged portion were practically sound and solid, while the intermediate, on which atmospheric evaporation had acted had absolutely disintegrated into a plastic mortar, apparently such as ordinary lime mortar when ready for plastering. This intermediate portion was a 1 to 5 mixture of sand and cement, and contained over 3 per cent. of sulphuric acid in the concrete itself, which would be equivalent to about 40 per cent. as much sulphuric acid salts as there was Portland cement present. We have concrete work of 1 to 6 mixture in cisterns and wells at our plant here at Portland which have stood in alkali water for over six years without a particle of injury, and yet wherever concrete of ordinary character has been acted on part of the time by the water and part of the time by the air it has shown signs of disintegration in a small way, so that the actual danger resides in the action of the atmosphere upon the sulphuric acid salts, which causes them in turn to act upon the lime of the cement changing it from the crystalline lime to the amorphous form, which not only swells but dissolves and is thus removed from the cement, causing its disintegration. The practical remedy in such a case would seem to be in the use of cement containing a very low percentage of lime, as this injurious action appears to increase rapidly with the increase of the excess lime of the Portland cement, and it has been found that any provision in the manufacture of Portland cement which assists in making the excess lime a permanent chemical component of a mineral will overcome this action of the sulphate salts.

The state of burning in the kiln, the fusibility of the clinker, the sluggish activity of the cement, all are accompanying characteristics of the cement which has large power in resisting the action of these sulphates, so that it is not to be doubted that when the demand shall arise the Portland cement manufacturers will find means of providing cement which will withstand the action of these alkalis in concrete if the cement is only given a fair show in the construction of the concrete. This is a matter which will require much painstaking, thorough, and unprejudiced experimenting before the whole proposition is made clear, and the means of obtaining the desired result is made positive.

SHEARING VALUES OF STONE AND CONCRETE.

By Henry H. Quimby.

A very common failure of stone lintels is by shear—rupture in a slightly diagonal direction up from one of the points of support. It is not at all as uncommon as it should be for the seats of lintels, and similarly loaded corners of brick work and concrete, to shear off, although the line of rupture is at a considerable angle with the direction of the stress. The seat of the proscenium arch girder of a well-known theatre some years ago sheared in this way and caused the girder to drop and wreck the roof which was carried by it. Reinforced concrete girders were designed and built somewhat extensively before their tendency to fail by shear was generally recognized and treated with special reinforcement. There was considerable and sometimes acrimonious controversy in public print between engineers holding opposite views as to whether or not shear as such actually existed in the girders and caused the failures in question. And with the advent of light concrete arches the question of the need for adequate resistance to shearing forces in an arch ring becomes pertinent.

The substitution of one material for another, such as of concrete for stone masonry, in any class of structure, may not unreasonably be expected to develop some failure to make provision for stress of a kind that with a previous material has not required special provision. In the common type of stone arches any shearing stress that there may be is so low per unit of section because of the great thickness of the haunches that it is more than abundantly cared for; besides, the shearing value of any stone that will be regarded as good enough for an arch is quite high compared with what is generally considered a safe compressive loading for stone masonry. Ordinary concrete however has a low shearing value and the fact must have consideration in designing structures to be built of it.

The shearing failure of lintel seats and arch rings is of the character of the usual compressive failure of a concrete prism or short column—a diagonal shear at an angle of anywhere from 35 to

40 degrees with the direction of the force of compression, and it is characteristic of a material of low relative shearing value. If in addition to the direct compressive stress in such a prism or column we apply force at one side deflecting the first force, unless the axis of the member be adjusted to coincide with the resultant of the two forces the side force will constitute a true shear, and if it be applied at the very point where diagonal rupture would occur from compression alone, it must have the effect of lessening the resistance of the prism by both increasing the component and causing the rupture to become more nearly tensile in character. If, as we try to do in an arch, we correctly adjust the axis of the member to the change in direction of the force, the resultant force delivered to the abutment skewback is theoretically a simple direct compressive stress resisted by a bearing that should be normal to it; but in practice it is rarely quite normal and never can be uniformly so because of the change in position and direction of the line of pressure due to variation in temperature and loading. A rocking and bending motion is thus induced and actual shearing stress is therefore clearly developed.

In certain recent tests of small model concrete arches such of them as were loaded in a manner to preclude buckling from eccentricity of stress failed by shear, one wherein the depth of ring was made uniform throughout punching itself down between the abutments, the line of shear being inclined up from the edge of the skewback. The action appeared like shear in a simple beam or lintel. Exactly similar failure occurred in a number of specimen prisms made and loaded to imitate the same condition of stress. It would therefore seem that the existence of shearing action at the edge of an arch abutment is sufficiently established to command attention to it and provision for it.

The compressive shear of the prism appears, by dividing the component along the line of rupture into the ruptured area, to show a unit shearing resistance of about four-tenths that of the unit direct compression. In a test for shearing values alone so high a result can be obtained only by some fanciful conditions of test that do not represent service. It is convenient to express the shearing value of concrete in terms of the compressive value, but there is not necessarily any relation between the two, because a prism might be made of flat plates not cemented together

and yet develop very high compressive resistance without having any tensile or shearing strength; yet if the pile be compressed at the same time by a direct pressure the friction between the plates will develop positive shearing resistance. This effect is supposed to obtain in ordinary concrete but is evidently more than offset by the lessened resistance to the compressive stress. The problem therefore becomes somewhat complex. Records of tests of the shearing quality of concrete are somewhat meagre and vary so widely that different investigators have evidently not only used widely different methods but have various ideas of what constitutes shear. The books variously give the shearing value of cement mortar and concrete, one as low as 16 per cent., another as high as 104 per cent. of the compressive value, one experimenter using cylindrical specimens and shearing them like a pin in eye-bars. Results obtained by tests can be intelligently used only with a clear knowledge of the exact circumstances, and some arbitrary allowance must always be made for what formerly was called personal equation but clearly goes far beyond that.

The character of shearing failures indicates the manner in which shearing tests should be made. The line of least resistance should be sought and the testing force applied there. As the shearing value seems to bear a fairly close relation to the tensile strength the line of rupture in a service failure is more or less diagonal. Tests made under fanciful conditions may be misleading. A shearing test of a granular material like concrete, stone, brick or mortar should not be made as if it were of steel. Academic tests in general are liable to be more interesting than practical.

It is very desirable that a designer should himself make the tests upon which he depends for his knowledge of the sustaining value of the material that he uses in important structures, for ideas develop under observation of the action of forces and the behavior of material.

As a general proposition a shearing test of concrete should have the cutting edges offset at least as much as the diameter of the largest particles of the aggregate in the specimen, and tests should record the facts of the amount of offset and thickness of specimen to give the angle of the line of rupture. Roughly the nearer this line is to 90 degrees—that is the closer the knives—the higher the

result. As the inclination increases the resistance decreases for the rupturing stress approaches tension in character.

A large number of recent tests of concrete and stone and brick seem to fix the most desirable angle of shear at one in eight—the upper cutting edge set back from the lower one-eighth of the thickness of the specimen,—and in the concrete tests this approximates the ratio between the thickness of the test piece and the diameter of the larger particles of the aggregates used in making them. Greater offset was found to be liable to cause failure by bending rather than by shear. The results obtained varied about as widely in each class as those of compressive and tensile tests of such materials do. The lowest from concrete, which was of pebble aggregate, was about 10 per cent. and the highest, which was of shale chip aggregate, was about 25 per cent. of the crushing strength. Crushed limestone aggregate, as also quartz sand mortar, gave an intermediate value. A fair quality of gneiss stone sheared at about 1,000 lbs. per sq. in., limestone at about 1,200; Conshohocken laminated mica schist at about 1,400; and Rushland black shale at about 2,500 lbs. per sq. in. A fair grade of red stretcher brick, such as is used for building houses, gave from 100 to 200 lbs. per sq. in. Medium burned shale brick from 400 to 900 lbs. per sq. in., and vitrified shale street paving brick 1,000 lbs. per sq. in.

The special rig designed for the investigation of the effect of shear in combination with compression, itself failed by shear, want of knowledge and consequent lack of proper judgment having invited disaster and interrupted the work. All of the partial tests made indicate that where shear operates in a member stressed in compression, as in the case of an arch ring, ordinary concrete at the ordinary unit-stress, does not give the margin that should be allowed. At the abutment skewback of a hingeless arch the compression in the arch is, or may be, resolved into horizontal and vertical components, and in the present knowledge of the science conservative practice treats these components as actual shear and provides for it with corresponding resistance in the arch section in horizontal and vertical lines.

In this view of the forces at work a concrete arch, especially of the lightened spandrel type that is being more and more widely adopted for both railway and highway bridges, unless it be clum-

sily heavy will have higher unit stress in it than modern practice warrants—when it knows it—for plain concrete. In a large arch with open spandrels the weight of the ring itself is a large part of the total load and therefore increase of section will be attended with such an increase of load that higher value cannot be thus obtained economically. The alternative is some species of reinforcement—not longitudinal but transverse. If steel be used for the purpose it should be horizontal in the column and radial in the arch. Longitudinal steel reinforcement is of doubtful value for any but tensile stress, but transverse reinforcement partakes of the nature of hooping and must therefore increase the compressive strength as well as the shearing resistance. The simple expedient of making a composite or hybrid material of concrete with embedded flat stone seems to meet these requirements of transverse reinforcement, and it has been adopted in a number of cases with very satisfying results. It is a combination of stone masonry and concrete, and it has been found to combine also increased strength with reduced cost. The process consists of placing layers of concrete mixed wet and sinking into each layer as many flat stones of any convenient size as can be embedded properly. Without special effort to secure it a very satisfactory overlapping for bond is obtained by the mere law of chance in depositing, and skilled masons are not required. This construction produces the most compact possible class of masonry. A large number of compression tests have been made on 6- and 12-in. cubes, using embedded stone having a shearing resistance of about six times that of the mortar of the concrete, and they gave from 30 per cent. to 50 per cent. higher ultimate crushing resistance than similar cubes of plain concrete. A number of shearing tests of small prisms made of this construction show even greater gain in shearing value. Certainly if the line of cleavage passes through the embedded stone the result will be high for concrete, provided, of course, that the embedded stone has a higher value than concrete, and stone that has not as high a shearing value as 1,000 lbs. should not be used for the purpose. The further facts that this hybrid construction increases the lateral tensile strength, which operates to resist settlement or other cracks, and reduces the shrinkage of the mass, thus minimizing internal stresses, are incidental but important.

The science of structural design is still incomplete, not because

the mathematics may be inexact but because the materials to which the mathematics must be applied are not yet thoroughly known as to their characteristics. We learn Nature's laws by running against them, not by reasoning, and therefore investigation is necessary to equip us for handling its products properly, and as the margin between success and failure—between safety and disaster—may be, and undoubtedly often is, very narrow, it behooves us to study thoroughly by tests all the peculiarities of our materials of construction.

PERMEABILITY TESTS OF CONCRETE WITH THE ADDITION OF HYDRATED LIME.

By Sanford E. Thompson.

These permeability tests were made to determine the effect of adding various percentages of hydrated lime to concrete of different proportions. The specimens were similar in a general way to those designed by the writer for previous tests, as described in the "Proceedings" of the American Society for Testing Materials, Vol. VI, p. 373, but were of much larger size; in fact, they were considerably larger than have hitherto been customary in permeability tests. The blocks of concrete were 20 inches in diameter by 16 inches thick, and the water was introduced into the center of the block as described and illustrated in this paper.

The proportions of concrete selected were those which are customarily used in concrete construction, except that a slight excess of sand was taken in proportion to the stone to insure homogeneous specimens, thus avoiding the possibility of stone pockets, which are more likely to occur in small specimens than in practical construction.

The materials, which are more fully described later in the paper, are quite representative, so that the results obtained are applicable to usual conditions in construction work. The cement was Lehigh brand; the crushed stone was conglomerate rock, which resembles trap in its characteristics and tests; the sand was a good, coarse bank sand. The hydrated lime was Pine Cone brand made by the Rockland-Rockport Lime Company, of Rockland, Maine.

The percentages of hydrated lime to be added were selected after a careful study of the materials and the proportions, the plan being to use larger percentages of lime with the leaner concrete mixtures, because these contained more voids to be filled. The proportions of concrete and the percentages of lime selected by this preliminary study were as follows:

Proportions of concrete.	Hydrated lime.	Proportions of concrete.	Hydrated lime.	Proportions of concrete.	Hydrated lime.
1 : 2 : 4	0	1 : 2½ : 4½	0	1 : 3 : 5	0
"	4%	"	6%	"	8%
"	7%	"	10%	"	14%
"	10%	"	14%	"	20%

The tests were laid out by the writer at the request of the Rockland-Rockport Lime Company, and were made by the Boston Elevated Railway Company, in their Cambridge laboratory. The scheme of tests was approved by Mr. W. E. Healey, of the Rockland-Rockport Company, and by Mr. George A. Kimball, Chief Engineer of the Boston Elevated Railway Company, and the results are here presented with their permission.

CONCLUSIONS.

The following conclusions are drawn as the result of the experiments:

1. Hydrated lime increases the water-tightness of concrete.
2. Effective proportions of hydrated lime for water-tight concrete are as follows:

For 1 part Portland cement	: 2 parts sand	: 4 parts stone, add	8%	hydrated lime.
" 1 " " "	: 2½ " "	: 4½ " " "	12%	" "
" 1 " " "	: 3 " "	: 5 " " "	10%	" "

These percentages are based on the weight of the dry hydrated lime to the weight of the dry Portland cement.

3. The cost of large waterproof concrete structures frequently may be reduced by employing leaner proportions of concrete with hydrated lime admixtures, and small structures, such as tanks, may be made more water-tight.
4. Lime paste made from a given weight of hydrated lime occupies about 2¼ times the bulk of paste made from the same weight of Portland cement, and is therefore very efficient in void filling.

Although the character of the sand and stone used in the concrete will affect the best percentage of lime to use, the present materials are representative of average materials throughout the country, so that the results should be of general application. Coarser sand would naturally require slightly larger percentages of lime, and finer sand, that is, sand having a larger percentage of fine grains which pass a sieve with 40 meshes to the linear inch, would be apt to require less lime, since sands containing considerable fine material produce a more water-tight although a weaker concrete.

A study of the curves which are given later in the paper sug-

gests that for coarser sands than used in the experiments, or in cases where the water pressure must be applied in one month or less after laying, the percentages given above may be increased by about 25 per cent., while for many classes of work 25 per cent. less than the above percentages will be satisfactory.

For mortar in proportions corresponding to the cement and sand in the different concretes, the same percentage of hydrated lime would naturally be selected; thus 1 : 2 mortar corresponds to 1 : 2 : 4 concrete, 1 : 2½ mortar to 1 : 2½ : 4½ concrete, 1 : 3 mortar to 1 : 3 : 5 concrete.

Increasing the proportions of stone to, say, 1 : 2 : 4½, 1 : 2½ : 5, or 1 : 3 : 5½, should have no effect on the percentage of hydrated lime to be selected, but for water-tight concrete it is always advisable to have a slight excess of mortar to avoid stone pockets.

RESULTS OF EXPERIMENTS.

The specimens ready for testing are shown in the photograph, Fig. 1.*

The water is introduced into the middle of the concrete cylinders. The pipe is enlarged as it enters the specimen so that an area of 12½ sq. ins., that is, a circle 4 ins. in diameter, is exposed to the water, and the water, to escape, must flow through 8 ins. of concrete.

Table I gives the results of the tests in detail, while the curves in Fig. 2 also present the effect of the hydrated lime upon the water-tightness of the concrete at the given ages.

The percentages of hydrated lime are based on the weight of the cement, these being added to the cement and not replacing it. The variation in the ages of the specimens for different proportions slightly affects the results and accounts in part for the fact that the 1 : 3 : 5 mixtures in the pressure tests are nearly as water-tight as the richer mixtures. Specimen No. 5, which shows practically no flow, is evidently erratic.

The specimens were first subjected to a head of 7 ft. by filling the vertical pipes with water, and while the results of these preliminary tests are of small value, because the pressure in most cases

* Acknowledgment is made to the *Engineering News* and the *Engineering Record* for the cuts used in this paper.

FIG. 1.—Permeability Specimens Ready for Test.

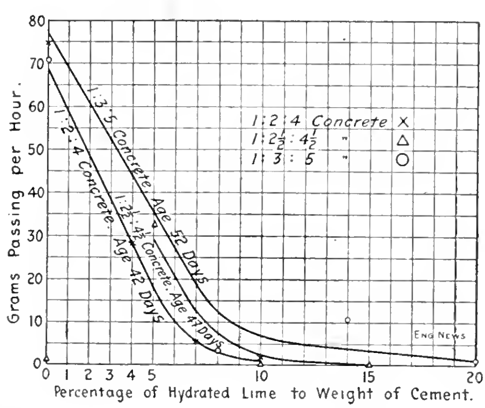

FIG. 2.—Average Curves showing Flow of Water through 8 ins. of Concrete with Varying Percentages of Hydrated Lime.

TABLE I.—PERMEABILITY OR FLOW OF WATER THROUGH CONCRETE 8 INCHES THICK.

		Flow of water under 7-foot head.			Flow under pressure of 60 pounds per square inch.			
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
No.	Hydrated lime per cent.	Age, days.	Duration of measured flow. hours.	Flow, grams per hour.	Age, days.	Pressure applied before measurement, hours.	Duration of measured flow, hours.	Flow, grams per hour.
1 : 2 : 4 *Concrete.*								
1	0	18	161	2.7	40	24	$4\frac{1}{4}$	74.8
2	4	18	161	1.2	41	18	5	28.4
3	7	18	161	1.0	42	18	$6\frac{3}{4}$	5.2
4	10	15	161	1.0	46	6	18	1.6
1 : 2½ : 4½ *Concrete.*								
5	0	30	169	0.3	44	17	6	1.1
6	5	30	169	1.9	45	18	6	32.5
7	10	29	169	0.8	49	..	21	0
8	14	29	169	0.7	50	..	27	0
1 : 3 : 5 *Concrete.*								
9	0	26	169	9.8	50	6	14	70.6
10	8	26	169	1.1	51	8	17	3.6
11	14	28	169	1.1	50	28	13	10.7
12	20	28	169	1.2	53	9	15	0.7

was not enough to force the water through the specimen, the quantity being measured before entering the concrete, the results are given in the table (Column 5) as confirming in a general way the tests of the concrete under city pressure. The tests under a 7-ft. head (Column 4) were maintained for several days, and the flow was recorded each day, the average only being given in Column 5.

After a longer period of setting, varying from 40 to 53 days, the specimens were connected with the city water pressure, which averaged 55 to 60 pounds per square inch. Before measuring the flow, the pressure was maintained for several hours (Column 7) to equalize the flow, and then the flow was measured for a period of several hours and recorded in grams per hour.

The flow given in grams per hour is very nearly the same as the flow expressed in ounces per 24 hours. The curves in Fig. 2 are plotted from the table.

The result from one specimen, the 1 : $2\frac{1}{2}$: $4\frac{1}{2}$ mix without any hydrated lime, is erratic, and this test is omitted in making up

the curves. All of the tests show some variations in the lower portions of the curves, but when the flow through the specimen is as small as 10 grams per hour, which is approximately equal to about 8 ounces or one-half pint per 24 hours, the concrete is so nearly water-tight that slight variations are unavoidable. The curves are based on a study of all of the tests. The pressure, nearly 60 lbs. per sq. in., which is equal to a head of about 140 ft. is unusual in engineering construction. Previous tests, by Fuller & Thompson (Trans. American Society of Civil Engineers, Vol. LIX, p. 133) have shown that the flow through concrete is almost directly proportional to the pressure, so that for lower pressure the flow will be correspondingly less. The thickness of the concrete was only 8 inches and water-tightness increases greatly with the thickness.

In selecting the best percentages of hydrated lime for the different proportions of concrete, these points, as well as the fact that the 1:3:5 specimens were slightly older than the others, have been taken into account, and as a result of this study proportions suggested for general practice are 8 per cent. for 1:2:4; 12 per cent. for 1:2½:4½; and 16 per cent. for 1:3:5 concrete, these percentages of lime being based on the weight of the cement. As stated, smaller percentages than these, for example, 6 per cent. for 1:2:4; 9 per cent. for 1:2½:4½; and 12 per cent. for 1:3:5, give results which should be satisfactory for many conditions in practice, while in certain cases a larger percentage may be chosen. Frequently it is advisable to make special studies or tests with the materials available to determine the best percentage.

The sand was ordinary New England bank sand and the crushed stone was conglomerate or pudding stone, whose quality and general characteristics for concrete are similar to trap.

The mechanical analyses of the aggregates are given in Table II, the sizes of grains being given as the total percentage passing the various sieves.

MAKING THE SPECIMENS.

The apparatus was designed before making the specimens, and full directions covering the details of the measuring and mixing of the materials and the making of the specimens were prepared in writing and sent to the laboratory of the Boston Elevated Railway Company, the tests being made by Mr. F. B. Edwards, Assistant

TABLE II.—MECHANICAL ANALYSES OF THE AGGREGATES.

Sieve.	Width of mesh of sieve, ins.	Total percentage passing Sand.	Total percentage passing Stone.
2 inch	2.00	......	100 %
1½ inch	1.50	......	92 %
1 inch	1.00	......	76.8%
½ inch	0.50	......	48.8%
¼ inch	0.25	100 %	12.6%
No. 5	0.16	97 %	1.8%
No. 12	0.0583	75.8%	1.8%
No. 20	0.0335	59.7%	1.3%
No. 40	0.0148	25.7%	1.1%
No. 100	0.0055	4.0%	0.7%
No. 200	0.0030	1.8%	0.4%

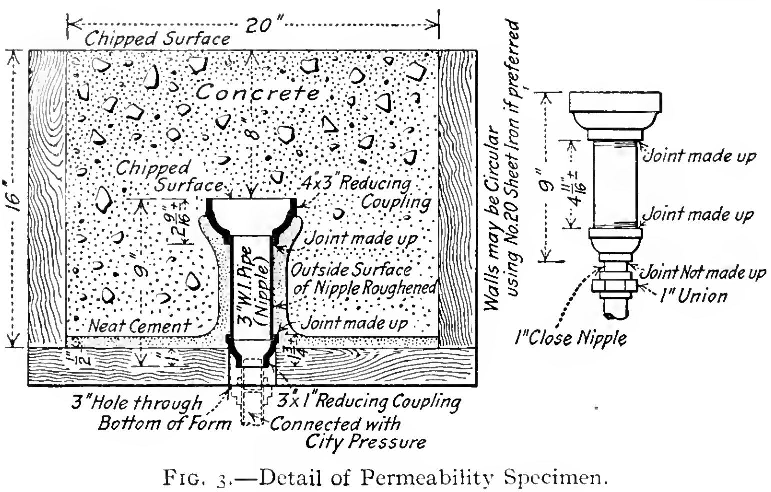

FIG. 3.—Detail of Permeability Specimen.

Engineer. A working drawing of the specimen, as molded with the apparatus required, is shown in Fig. 3. Instead of using a wooden mold, the sides were formed by two pieces of wrought iron bent to half-circles and bolted together. These rested on a plank platform which formed the bottom of the mold until the concrete had set.

The proportions were based on volume measurements, assuming 100 pounds cement to the cubic foot, but for greater accuracy in making the tests the weights per cubic foot of the sand and stone

were determined and the measurements made by weight. The cement and hydrated lime were first thoroughly mixed to a uniform color by passing several times through a $\frac{1}{8}$-inch sieve; then the sand was added and mixed dry. The stone was added finally and sufficient water used to make a soft, mushy mix, such as would be adopted in construction. The mixing was very thorough, but no more so than would be expected in practice with the ordinary types of concrete mixers.

Since previous tests by the writer showed that the top of a block as molded is much more water-tight than the bottom, because of the fine material which rises to the surface, the specimens were made in an inverted position, so that Fig. 3 shows the specimen upside down when compared with its position in the permeability test. The iron pipe fittings were placed upright in the molds, as shown, fittings which were easily obtainable being selected to cheapen the cost. These were so placed as to give a thickness of 8 ins. of concrete in all directions, and a surface for the water to strike against of $12\frac{1}{2}$ sq. ins., that is, a circle 4 ins. in diameter. The fittings were filled with sand before placing the concrete, and the top was covered with oiled paper to prevent the concrete getting into the pipe.

The bottom of the mold was covered with a thin layer of neat cement, about $\frac{1}{2}$ in. thick (instead of 1 in. as given in the drawing), and neat cement was also plastered around the fittings, which had first been thoroughly cleaned and scraped with a brush, thus forming a perfectly tight joint. There was no leakage in any of the specimens; in fact, it would appear that it is unnecessary to insert the pipe so far into the specimen, 2 or 3 ins. probably being sufficient.

When the concrete had hardened, the sand and oiled paper were removed from the pipe and the surface chipped free from mortar down to the clear concrete, the large size of the fittings permitting a chisel to be readily worked. The top of the specimen as molded was also chipped down so that the stones were exposed in the surface of the concrete. When ready to be tested, each specimen was supported by 2 x 4-in. pieces of studding, which are shown in Fig. 1, and the fittings were connected with a pipe passing up through the ceiling to the floor above. Before connecting

the fittings were partly filled with sand, thoroughly washed and screened to about 30-mesh size. A funnel 24 ins. at the top reducing to the size of the neck of a large bottle was placed below each specimen to assist in collecting the water.

The concrete in each specimen weighed over 400 lbs.

PROPORTIONING THE HYDRATED LIME.

Few tests have been made to determine the effect of the introduction of special materials into concrete to increase its water-tightness. The percentages of hydrated lime to be used were determined by applying the conclusions derived from tests with graded concrete aggregates and the results of the tests indicate that mechanical analysis methods can be applied to the laying out of permeability tests.

For the 1:2:4 proportions the percentages of hydrated lime were selected partially by judgment. From previous tests by the writer, 2 per cent. of hydrated lime had been found to increase the water-tightness of 1:2:4 concrete made with sand and gravel, and 4 per cent. of lime to still further increase it. The minimum percentage was therefore selected as 4 per cent., and the maximum percentage for the 1:2:4 mix was taken as 10 per cent. so as to confine the results to comparatively narrow limits. One intermediate percentage was also chosen, thus giving 0, 4, 7 and 10 per cent. respectively for the 1:2:4 concrete.

Leaner concrete mixtures evidently require more fine material for water-tightness, because there are more and larger voids. Furthermore, tests in general indicate that with a lean mix we can add much more fine material without reducing the strength, and that this addition up to a certain limit is beneficial not only for water-tightness but also for strength.

For selecting percentages of lime for the 1:2½:4½ and the 1:3:5 concrete which would compare with the percentages selected for the 1:2:4, methods of mechanical analysis were employed, the object being to obtain mixtures for each of these other proportions which would have substantially the same sizes of particles. In Fig. 4 the mechanical analysis of the cement, sand and stone are plotted and a combination curve of these three materials made for 1:2:4 and 1:3:5 concrete. The combined

curves are simply the mechanical analyses of the mixture of the three dry materials in the given proportions.*

The curves of the individual materials indicate the percentages passing the sieves of various sizes. For example, taking the curve for the broken stone, which is the lower curve, all the stone, or 100 per cent., passes a sieve 2 ins. in diameter, 92 per cent. passes a $1\frac{1}{2}$-in. sieve, 76.5 per cent. a 1-in. sieve, and so on. The sand and cement curves are plotted in a similar fashion.

Having plotted these individual curves, it is possible to cal-

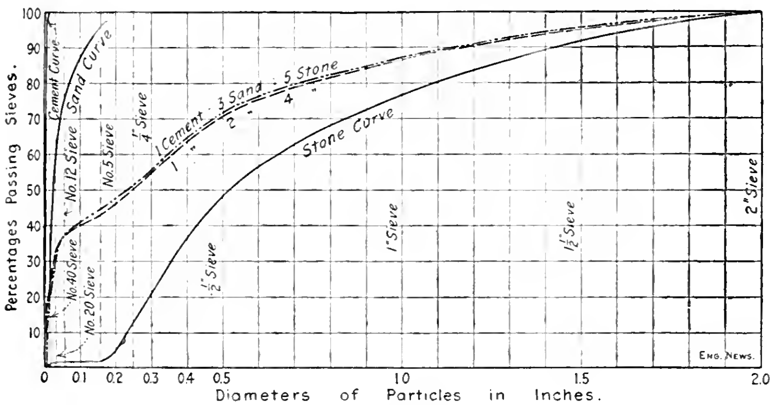

FIG. 4.—Mechanical Analyses of Concrete Materials and Mixtures.

culate the percentages and plot combined curves showing mixtures of these three materials in any given proportions.

The making of these combined curves is quite simple. Thus, for example, to find the point on the 1 : 2 : 4 curve where it cuts the ordinate corresponding to the No. 12 sieve, the sums of the percentages of the individual materials at this same ordinate are taken in the proportion which they bear to the concrete mixture. All the cement is finer than the No. 12 sieve, and since the cement is one part of the seven parts in the mixture, one-seventh of 100 per cent.

* For description of mechanical analysis methods by William B. Fuller, see Chapter XI, "Concrete, Plain and Reinforced;" also paper on "The Laws of Proportioning Concrete," by Fuller & Thompson, in *Transactions American Society Civil Engineers*, Vol. LIX, p. 67.

represents the percentage of cement in the mixture at the given ordinate. Similarly, since there are two parts of sand in the seven parts, the sand percentage at the No. 12 ordinate, 76 per cent., is multiplied by two-sevenths, and the stone percentage, 1.8 per cent., by fourth-seventh, thus giving as the point on the No. 12 sieve ordinate in the combined curve:

$$
\begin{array}{lll}
1/7 \times 100 \text{ per cent.} & = & 14.3 \text{ per cent. for cement} \\
2/7 \times 76 \text{ per cent.} & = & 21.7 \text{ per cent. for sand} \\
4/7 \times 1.8 \text{ per cent.} & = & 1.0 \text{ per cent. for stone} \\
\hline
 & & 37.0 \text{ per cent. for } 1:2:4 \text{ mix.}
\end{array}
$$

The other points in the curves are found in a similar manner.

Permeability depends more upon the gradation of the fine particles than of the coarse, and the left-hand portions of the curves, representing the finer material, are therefore replotted in Fig. 5 to larger scale. Referring to the curves for 1:2:4 and 1:3:5 proportions, we notice that they are quite similar above, that is, to the right, of the No. 20 sieve, and that the greatest proportionate difference between them is below the No. 40 sieve. We may assume therefore that the particles lying below the No. 40 sieve are those which chiefly affect the water-tightness. Therefore, the finer sieves, No. 100 and No. 200, must be chiefly used in studying the percentages of lime to be added.

Several percentages were tried, and it was found that to make the 1:3:5 curve most nearly similar to the 1:2:4 concrete, 10 per cent. of lime must be added to the 1:3:5 mix. The curves of these two mixtures, together with the curves of concrete with no lime, are shown in Fig. 5. The other proportions were also plotted in the same way, but are omitted in the diagram for the sake of clearness.

The intermediate proportions for the 1:3:5 mix were thus determined upon as 8 and 14 per cent., while for the $1:2\frac{1}{2}:4\frac{1}{2}$ proportions, 0, 6, 10 and 14 per cent. respectively were found to correspond with the selected proportions for 1:2:4. Since a given weight of hydrated lime makes more than twice as much paste as the same weight of cement, the lime percentages are doubled when combining them with the other curves.

PREVIOUS TESTS WITH HYDRATED LIME.

A few permeability tests with hydrated lime admixtures made by the writer in 1903 indicated it to be a valuable material for waterproofing. Later, in 1906, when the reservoir at Waltham,

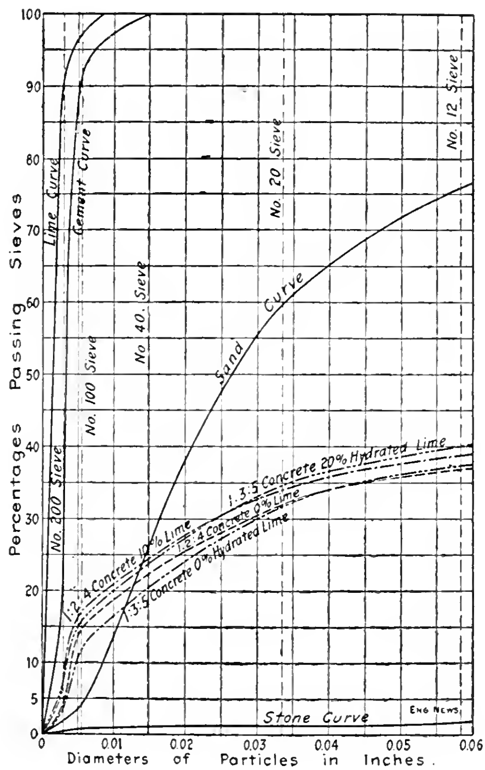

FIG. 5.—Mechanical Analyses Curves with Hydrated Lime Admixtures.

Mass.,* which is 100 feet in diameter and 43 feet high, was under consideration, the writer was consulted by Mr. Bertram Brewer, City Engineer, in the framing of the specifications and made another series of tests, as follows:

* See *Engineering Record*, January 12, 1907, p. 32.

PERMEABILITY TESTS OF 1 : 2 : 4 CONCRETE FOR WALTHAM, MASS., RESERVOIR, 1906.

Concrete, 4 inches thick. Pressure, 80 pounds per square inch.

Percentage of hydrated lime.	Flow in grams per minute (maximum flow)		
	At 14 days.	At 21 days.	At 28 days.
0 per cent.	5.52	2.92	1.91
2 per cent.	9.20	2.55	1.63
4 per cent.	2.82	1.49	0.76

Percentage of lime is in terms of weight of cement.

As a result of these tests, 5 per cent. of hydrated lime was adopted to mix with the 1 : 2 : 4 concrete in building its walls. The results were satisfactory, the only seepage occurring at some of the joints formed between different day's work where the bond between the old and new concrete was not made with sufficient care.

CONSTRUCTION OF WATER-TIGHT CONCRETE.

A paper on permeability is incomplete without reference to the general principles of construction which must be followed to produce a water-tight concrete. The concrete materials may be perfectly graded and the proper proportions of cement and of hydrated lime used, and yet if the concrete is poorly mixed, mixed with insufficient water, or improperly placed, or if joints are left in the mass, the wall will invariably leak.

In the first place, the mixture must be thorough, and in the second place, sufficient water must be employed to give at least a "mushy" mix, so that it will settle into place with only a small amount of ramming.

Fully as important as the mixture is the bonding of the concrete between two days' work. For a small structure which must be water-tight, it is advisable to place the concrete continuously, allowing no joint whatever, and not even permitting the concrete to stiffen up between the batches. Even an interruption of an hour in the middle of a hot day has been known to form a joint which will allow water to pass. If continuous work is impracticable, the old surface of the concrete must be thoroughly cleaned of all dirt and laitance or partially set cement, so as to expose the concrete. A layer of neat cement paste of soft con-

sistency, or else of 1 : 1 mortar, should be then spread upon the old concrete after thoroughly wetting it, and the new concrete laid before this mortar has stiffened. The placing of the mortar or paste does not preclude the necessity of thoroughly cleaning the surface, for if any of the laitance or partially set cement is left on the old concrete, it will form a joint under the new mortar through which water will pass.

A long wall or a conduit may require longitudinal reinforcement to prevent temperature cracks and therefore leakage through them.

TESTS OF REINFORCED CONCRETE BLOCK SEWER AND RAILWAY CULVERTS.

By Burton Lowther.

During the writer's experience as engineer on railway construction, the need of a system by which culverts and small waterways could be constructed quickly and cheaply became apparent.

After much experience with stone masonry, clay pipe, cast and wrought iron pipe, it was found that concrete was the ideal material for such work, except for the cost of forms and labor, and the delays often experienced in completing the grading of the road-bed. This is especially true in electric railway construction and light branch lines of steam railway.

The rapidity and cheapness of construction of some concrete block houses, built about this time, drew to the writer's attention the possibilities of a system of arch sections of cured concrete blocks, properly designed and reinforced to meet the conditions peculiar to railway construction. As this system could also be applied to sewer construction, and from the fact that the most interesting case of test was developed from a sewer section, this example will be given.

This section was egg-shaped, as shown in Fig. 1, the size being 48 x 72 ins. The mixture used consisted of crushed rock, passing a 1-in. ring, a good quality of river sand, and Portland cement, proportioned 1:2:4.

Each section consisted of four pieces, one invert, two sides, and one arch or crown block. Each block was 24 ins. wide. The crown block was 4 ins. thick, the thickness of the blocks gradually increasing from the center of the crown block to the invert, which had a maximum thickness of 11 ins. The side blocks lapped or staggered half their width, or 12 ins., on the two adjacent invert blocks. The bottom of the invert block was reinforced with three ½-in. medium steel rods, spaced 8 ins. apart. These were placed near the flow line of the invert, and bent up along the sides of invert-block to a point near the top. The middle rod, which was placed in the center of the block,

projected about 4 ins., and the ends formed loop-eyes to which were attached the reinforcing rods of the sides and the arch block, so that the latter, acting as a beam, would resist the bending moment produced by the thrust of the side blocks.

This section of 48 x 72-in. sewer was set on roughly level earth without any special attempt at leveling, thus assuring average conditions of actual practice in the test.

A rough plank platform, with 2-in. bearing blocks, spaced about 6 ins. apart, to transmit the load to the crown block, was placed on top of the section, as shown in Fig. 2. The load consisted of pig iron and heavy foundry castings, forming a prac-

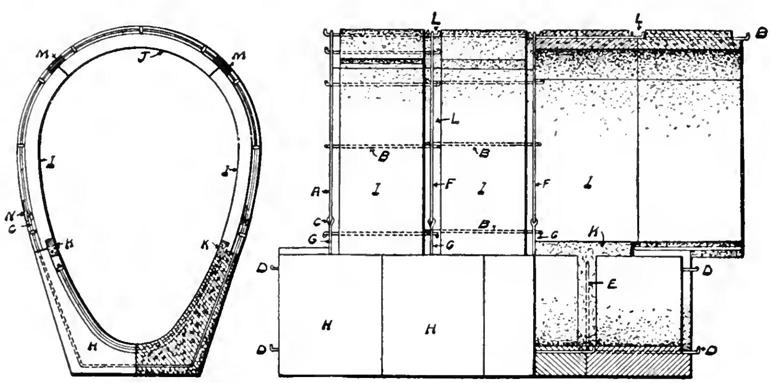

FIG. 1.—A, reinforcing rod connecting side and arch blocks to invert at *C*; *B*, lateral reinforcing rod interlocking side and arch blocks with invert reinforcing rod *G*; *C*, loop eye in rod; *D*, lateral reinforcement of invert connecting invert blocks and engaging rod *E* of invert; *H*, invert blocks; *I*, side blocks; *J*, arch blocks; *K* and *M*, grout of cement mortar sealing reinforcement; *L*, grout chambers for sealing reinforcement and making water-tight joint; *M*, grout in place.

tically solid mass of iron, uniformly distributed over the platform. The deflection under progressive loading was obtained by actual measurement, referred to a solid base so arranged that the slightest movement, laterally or horizontally, could be detected. The load was placed on the platform from an elevated platform by laborers, without much regard to the shocks produced by the falling of the iron.

No deflection was observed up to a loading of 10,000 lbs. and the joints of the sections remained without a crack. A deflection of $\frac{1}{32}$ in. was observed under a load of 15,000 lbs.; at 19,500 lbs. the deflection was $\frac{1}{16}$ in. At 25,000 lbs. a deflection of $\frac{3}{32}$ in. was observed and a fine crack developed on the outer or tension side of both side blocks, running the full length of the sides. An

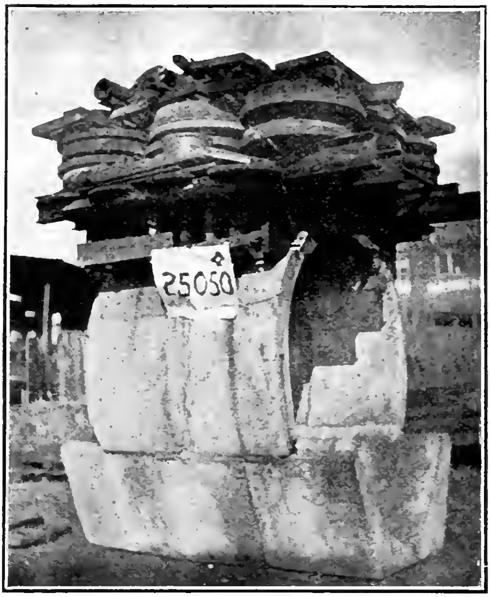

FIG. 2.

additional load caused a crack at the flow line of the invert, the latter failing as a beam over the entire length of invert.

It will be noted that the failures occurred at the quadrant points of the curve of the section. The joints remained throughout the entire test free from movement. These joints, it will also be noticed, are placed at the points of least bending moment, viz., at points 45° from the axes of the cross-section.

The advantages claimed for this system are as follows:

1. There is an absolute mechanical bond of the reinforcement in all directions, as in monolithic concrete work.

2. The construction is more rapid than any other method,

since no forms are required, the sections being self-aligning and self-supporting.

3. The trench can be filled at once after grouting is completed.

4. The inside surface is smooth and true, giving a greater discharge for a given cross-section area than brick sewer or concrete sewer built with forms.

5. The block sections can be inspected before they are put in place and if any are defective they can be rejected, thus insuring the most reliable work.

6. The quantity and character of the reinforcement can be varied to meet the conditions of loading, grade, soil, etc.

7. This construction can easily be handled in wet, springy soils, and where running water is encountered in the trench.

It is the belief of the writer that the use of cured concrete for structural uses will become general, introducing an era of more economical design and more reliable construction. There seems to be no reason why concrete building members, such as columns and beams, should not be made in shops and delivered to the building site, just as structural steel members are. This would allow a test of specimens representing each structure, thus guaranteeing absolute safety.

THE INFLUENCE OF THE ABSORPTIVE CAPACITY OF BRICK UPON THE ADHESION OF MORTAR

By D. E. Douty and H. C. Gibson.

This investigation, of which this is but a preliminary report, was originally undertaken at the request of one of the large brick manufacturing companies supplying the trade through the Southern and South Atlantic States.

Failure in bonding, and the ultimate cracking and separation of the facing and filler bricks in a building in one of the larger southern cities, led to a claim, on the part of the contractor that the "non-absorbent" brick supplied by the manufacturer was responsible for the defect. The contractor maintained that the adhesion of mortar to bricks of small absorptive capacity was very much less than to those of large absorptive capacity.

Agreeing that scientific evidence carefully secured by a neutral party would be more satisfactory and equitable in adjusting their differences than action at court, the matter was referred to the Bureau of Standards with the request that tests be made on samples of the brick in question. The tests upon the specimens furnished yielded some interesting and satisfactory, although not entirely concordant results, and it was decided to extend the investigation beyond its original proportions to a greater variety of specimens furnishing a wider range of absorptive capacity.

Several other manufacturers were asked to coöperate and the response was so generous that we were soon supplied with several thousand bricks comprising twenty-three varieties.

It is the preliminary work on these twenty-three varieties that we propose to present to you. We shall be pleased to receive your frank criticism and any suggestions which you will offer us in the extension of the work.

During recent years the very rapid increase in the use of cement in construction work has compelled and developed a corresponding increased activity on the part of the brick manufacturers to improve their products in order to meet the severe competition and growing favor of cement in various forms.

The engineering journals and the transactions of engineering, manufacturing and architectural societies in this country and abroad, in fact the entire domain of engineering literature has for the past twenty years been overflowing with the results of investigations into the physical properties and chemical relations of cement and cement products. Societies, institutions and individuals have vied with one another in attempts to secure reliable data upon which to base *standard* cement specifications, *standard* methods and instruments for testing, and to secure uniform conditions and regulations. This immense amount of labor has produced the result, in the securing of which this Society has had a very large share, that out of the chaos of a few years back, we now begin to see uniformity and harmony emerging. The construction engineer who does not buy, judge and accept his cement and cement products by *standard* specification and *standard* tests by which the physical properties of his materials are definitely determined, must soon yield his claim to the first rank.

In the case of building bricks it is quite different. As a material for construction they are of very ancient origin. Ceramics is one of the oldest handicrafts of man and its products have been among the most enduring. We have so long associated bricks with the building trades that we now seem quite content to look at them, strike them with a hammer or trowel, break them across one another and put them into our buildings. Consumers and construction engineers have given very little attention to securing standard methods of testing. The manufacturers, through the National Brick Manufacturing Association have made some progress and in the matter of paving brick have accomplished much.

Some valuable results have been contributed by Mr. H. J. March, Prof. Marston of Iowa State College, Prof. H. A. Wheeler, Prof. Edward Orton, Ohio State University, Prof. Talbot, Prof. Hatt of Purdue University, Prof. Woolson and others. Either in the course of their reports or in the discussion of their results nearly all writers express regret on account of the small amount of work which has been done and the lack of *standard, uniform* methods which would make comparison possible. In fact brick testing is in about the condition that cement testing was fifteen or twenty years ago.

ABSORPTION.

In selecting a method for determining absorptive capacity, we found several available, but quite a wide difference of opinion as to their dependability.

Prof. H. J. March* used total immersion for 48 hours, making tests upon half brick, bricks with ½ in. of the outer surface removed, and whole brick. He states that "the absorption percentage upon similar whole bricks was less than either of the others, showing that the interior of the brick was more porous than the exterior and also indicating, since it was a 48-hour immersion, that 24 hours immersion would be too short a time"—although, of course, the greater amount of absorption would occur in the first few hours.

Prof. Wheeler† says "it is impossible to obtain any figures representing the total porosity of bricks by the absorption test in any reasonable time, that the rate of absorption of different bricks varies enormously, and that at any short period of time such as would be available for a commercial test, the results of the test would be misleading."

Prof. Woolson‡ carried on a series of tests in 1905, upon which the *Regulations* of the Bureau of Buildings of New York are now based, in which absorption is determined by immersing to a depth not exceeding ½ in. and weighing at periods of 30 minutes, 4 hours, and 48 hours. These tests were made on a limited number of samples, representing a limited range of absorptive capacity, and Prof. Woolson expresses at the close of his paper the great desirability of more extended observations.

We have made determinations by four different methods extending over a maximum period of 17 days as follows:

1. Total immersion.
2. In contact with a saturated felt.
3. Partial immersion to a depth of ½ in.; (*a*) covered, (*b*) exposed.
4. Boiling.

As we were not interested in the rate of absorption but in the

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* *Brick*, August, 1901, p. 56.

† *Brick*, September, 1901.

‡ *Engineering News*, 1905.

TABLE I.—PERCENTAGE ABSORPTION BY TOTAL IMMERSION.

Sample No.	Time, Days. 1	2	3	4	5	6	10	17
1	7.74	8.06	8.27	8.46	8.63	8.63	8.81	8.83
2	9.32	9.53	9.83	9.97	10.17	10.29	10.45	10.60
3	11.63	11.96	12.25	12.35	12.46	12.60	12.83	12.83
4	9.52	9.73	10.09	10.15	10.36	10.48	10.55	10.66
5	7.21	7.50	7.69	7.76	7.89	7.94	8.06	8.22
6	7.96	8.36	8.56	8.67	8.76	8.90	9.11	9.25
7	5.31	5.66	5.87	6.04	6.16	6.31	6.55	6.89
8	6.44	6.85	7.07	7.21	7.36	7.50	7.68	7.89
9	5.96	6.28	6.47	6.59	6.67	6.72	7.06	7.27
10	0.65	0.63	0.63	0.63	0.68	0.71	0.70	0.68
11	0.72	0.70	0.71	0.72	0.74	0.75	0.77	0.70
12	1.28	1.28	1.29	1.28	1.35	1.36	1.38	1.39
13	8.69	10.24	10.41	10.45	10.63	10.64	10.78	10.84
14	7.23	7.60	7.79	7.91	8.09	8.20	8.35	8.49
15	1.69	1.80	1.82	1.81	1.96	1.96	2.05	2.16
16	4.47	4.67	4.73	4.84	5.00	5.13	5.28	5.48
17	5.49	5.80	5.97	6.15	6.26	6.37	6.68	6.90
18	5.92	6.14	6.14	6.25	6.36	6.49	6.63	6.78
19	5.08	5.20	5.25	5.43	5.55	5.67	5.71	5.83
20	8.82	8.91	8.88	9.01	9.13	9.29	9.47	9.68
21	8.07	8.12	8.27	8.44	8.61	8.64	8.89	9.16
22	2.42	2.55	2.66	2.81	2.88	2.93	3.06	3.14
23	5.75	5.90	5.99	6.18	6.23	6.34	6.45	6.59

NOTE.—Each value is the average of three tests.

total absorption and hence the determination of the least time required to obtain reliable results, weighings were not made, except in a few cases, for periods of less than one day.

1. *Total Immersion.*—As the total immersion method is the one most used we have made more extensive and complete observations by it. As is usual in absorption tests the specimens were first dried in an air bath at a temperature sufficient to vaporize the moisture 110°—120° C., and until successive weighing showed that they had reached a constant weight. They were then immersed in water at room temperature, and weighings were made every 24 hours for six days and on the 10th and 17th days. The results were computed on the basis of the dry weight and those tabulated, Table I are the averages of three specimens of each sample.

The chief objection to the method is the lack of a definite termination of the absorption. This is probably due to the water forming a seal around the smaller cavities and only permitting the air to escape slowly. In Fig. 1 the increase of the absorption with the time is shown graphically for Nos. 3, 6, 10, 15, and 17,

representing the entire range from greatest to least absorptive samples. It will be noted that even after 17 days the curve, in most cases, is still rising and the increase during the first five days is very marked.

2. *Wet Felt Method.*—Hoping to imitate more nearly the conditions to which exposed bricks are subjected in use, a series of weighings was made of another lot of specimens which were

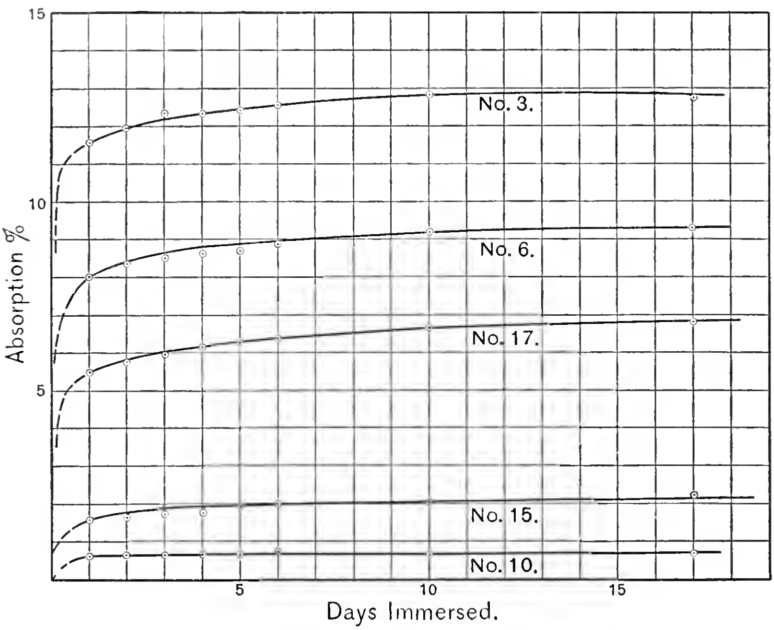


FIG. 1.—Showing the increase of absorption with time.

laid with one broad surface in contact with a heavy hair felt kept saturated in a tank with constant water level arrangement. By this means absorption was accomplished by capillary action alone against gravity, and it was hoped that more of the air would be driven out, especially in the more compact samples, and more uniform results obtained. The results are as in the first method, the average for three specimens. The values obtained, Table II, are very nearly the same as those by total immersion, being in some cases slightly greater, in others slightly less.

TABLE II.—PERCENTAGE ABSORPTION ON SATURATED FELT.

Sample No.	Time, Days. 1	2	3	4	5	6	10	17
1	6.51	6.86	7.05	7.13	7.21	7.35	7.50	7.75
2	7.46	8.08	8.33	8.44	8.52	8.72	8.83	9.13
3	12.15	12.57	12.77	12.85	12.88	13.02	13.13	13.42
4	9.52	9.93	10.15	10.24	10.32	10.40	10.54	10.77
5	6.79	7.20	7.40	7.46	7.54	7.63	7.77	7.97
6	8.14	8.57	8.77	8.86	8.94	9.04	9.20	9.46
7	3.85	4.39	4.85	5.10	5.14	5.30	5.48	5.88
8	6.64	6.79	7.04	7.19	7.25	7.39	7.68	7.97
9	5.47	5.87	6.08	6.18	6.25	6.71	6.86	7.09
10	0.18	0.25	0.31	0.34	0.34	0.37	0.58	0.65
11	0.37	0.46	0.57	0.60	0.59	0.60	0.64	0.70
12	0.70	0.88	1.00	1.00	1.02	1.03	1.07	1.15
13	8.87	8.90	9.21	9.27	9.32	9.47	9.55	9.75
14	5.06	5.45	5.70	5.83	5.95	5.99	6.15	6.36
15	0.58	0.76	0.87	0.92	0.95	1.00	1.09	1.29
16	3.37	3.73	3.94	4.05	4.10	4.21	4.39	4.60
17	5.22	5.71	6.14	6.31	6.43	6.53	6.71	6.95
18	5.64	5.90	6.05	6.12	6.16	6.24	6.36	6.69
19	5.27	5.50	5.65	5.74	5.78	5.86	5.98	6.18
20	8.53	8.80	8.96	9.02	9.07	9.16	9.19	9.50
21	7.54	7.71	7.90	8.04	8.46	8.23	8.45	8.68
22	2.22	2.54	2.72	2.83	2.89	2.93	3.05	3.29
23	5.91	6.16	6.30	6.41	6.52	6.63	6.75	6.93

NOTE.—Each value is the average of three tests.

The specimens were kept in an open tray exposed to the varying thermal and humidity conditions of the room which, in the light of some later observations, appears as a mistake, but it was not discovered until too late to make the change, and lack of time has not permitted us to repeat the observations under more constant conditions.

3. *Partial Immersion to a Depth of ½ in.*—The two previous methods having indicated to us the range of absorptive capacity available, we selected six samples representative of the series and tested them according to the method suggested by Prof. Woolson. They were dried to a constant weight, then laid on their broadest surface in a tray containing water to a depth of about ½ in. One-half were covered to prevent a circulation of air; the other half were left exposed.

As the observations are not complete we are unable to give results (Table III) beyond the third day. In most cases the total absorption is greater for the covered specimens than it is for the exposed ones.

TABLE III.—PERCENTAGE ABSORPTION, IMMERSED ½ INCH.

Sample No.	4 Hours.		24 Hours.		72 Hours.	
	Open.	Closed.	Open.	Closed.	Open.	Closed.
2	6.04	8.56	7.54	9.38	8.21	10.05
3	11.15	10.29	11.28	11.29	12.62	11.80
8	5.61	5.60	6.29	6.43	7.04	7.12
10	0.38	0.37	0.58	0.44	0.69	0.49
12	0.73	1.14	0.96	1.18	1.05	1.21
16	2.37	4.05	3.20	4.86	3.45	5.15

4. *Boiling.*—As stated above, it seems very probable that the entrapped air is responsible for much of the variation in absorption tests and the length of time required to obtain a maximum. This can, of course, be avoided by allowing the absorption to take place in vacuum, but a method depending upon the exhaustion of the air would require considerable apparatus available only in an equipped physical laboratory, and hence would not be as practical as a simpler method requiring only facilities easily obtained. It occurred to us that boiling might possibly furnish an accelerated test which, although quite foreign to normal conditions in use, would serve to drive out the entrapped air and yield more uniform results.

As in the third method the results are only fragmentary and suggestive. The stress of work during the government-contract season has left us only sufficient time to make a few hasty observations.

The samples in a dry condition were placed in a water bath and boiled for four hours. They were then allowed to cool and a few hours later were weighed. After twenty-four hours they were again boiled, cooled and weighed. They were then allowed to remain immersed for 48 additional hours, boiled for about an hour more, allowed to cool and were weighed.

TABLE IV.—PERCENTAGE ABSORPTION BY BOILING.

Sample No.	Time Immersed.			Increase, 72 Hours over 4 Hours.	Difference between Total Immersion and Boiling.
	4 Hours.	24 Hours.	72 Hours.		
3	14.53	14.65	14.71	0.18	2.46
16	6.73	6.81	6.79	0.06	2.06
10	2.38	2.42	2.42	0.04	1.79

All samples showed uniform increased absorption (Table IV) over the values obtained by all other methods of approximately 2 per cent.

In No. 3, the most absorptive sample the increase after the first boiling was only 0.18 per cent. while by the partial immersion method it was 1.47 and 1.51 per cent. respectively, open and closed.

Likewise No. 16, the one of medium absorptive capacity,

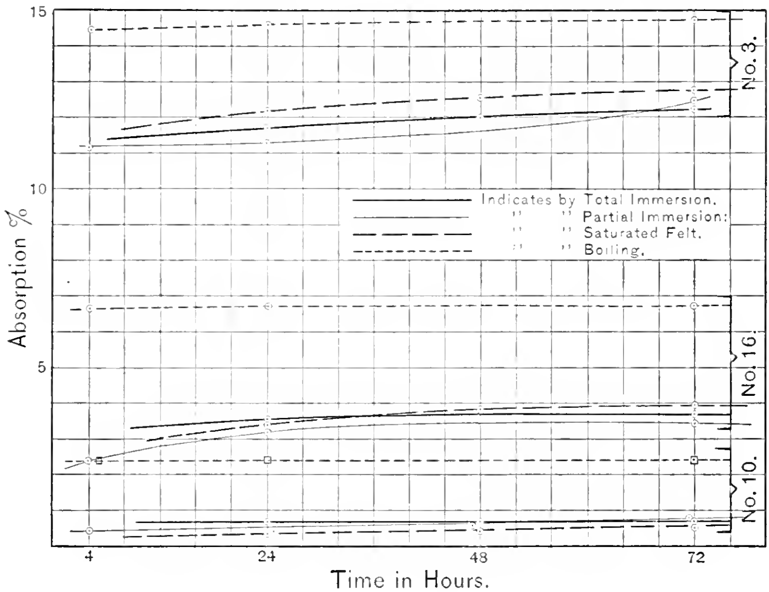


FIG. 2.—Showing the increase of absorption with time, determined by four different methods.

showed an increase of 0.06 per cent. as against approximately 1 per cent. by the other method. And No. 10, the least absorptive, showed but 0.04 per cent. as against 0.31 and 0.12 per cent. by the other method.

It remains to extend the test to a larger number of samples, return the samples to their dry condition to determine if any of the material has gone into solution and ultimately to compare the results with values obtained by conducting the absorption at reduced pressures. The values for absorption obtained by the

TABLE V.—PERCENTAGE TOTAL ABSORPTION, 3 DAYS.

Sample No.	Total Immersion.	Wet Felt.	Immersion ½ inch.		Boiled.
			Open.	Covered.	
1	8.27	7.05	......	......	......
2	9.83	8.33	8.21	10.05	......
3	12.25	12.77	12.62	11.80	14.71
4	10.09	10.15	......	......	......
5	7.69	7.40	......	......	......
6	8.56	8.77	......	......	......
7	5.87	4.85	......	......	......
8	7.07	7.04	7.04	7.12	......
9	6.47	6.08	......	......	......
10	0.63	0.31	0.69	0.49	2.42
11	0.71	0.57	......	......	......
12	1.29	1.00	1.05	1.21	......
13	10.41	9.21	......	......	......
14	7.79	5.70	......	......	......
15	1.82	0.87	......	......	......
16	4.73	3.94	3.45	5.15	6.79
17	5.97	6.14	......	......	......
18	6.14	6.05	......	......	......
19	5.25	5.65	......	......	......
20	8.88	8.96	......	......	......
21	8.27	7.90	......	......	......
22	2.66	2.72	......	......	......
23	5.99	6.30	......	......	......

four methods are collected in Table V. They are all the averages of determinations made on three specimens of each sample, and since by the third and fourth methods, weighings were not made after the third day, they include only that period.

In Fig. 2 the results for Nos. 3, 10 and 16 are shown graphically. The dotted lines indicating the values obtained by boiling are nearly straight and horizontal while most of the other curves show a decided increase with time.

ADHESION TEST.

There has not been a large amount of work done upon the adhesion of mortar to brick and the conditions which influence it. The International Society for Testing Material has had Committees at work upon it at intervals since the conference at Munich in 1884.

M. Feret presented at the Brussels Congress, in 1906, a report on "The examination and evaluation of the Resolution of the

Conferences of 1884–1893 over 'The Adhesive Strength of Hydraulic Cements.' " This work has had as its principal object the development of a standard surface and method by which the adhesive strength of different cements and cement mortars may be compared.

We have been interested in the measurement of the adhesive strength of the same mortar to different specimens and in finding if any relation exists between the adhesive strength and absorptive capacity. As the work is still in progress I am able to report only a portion of it at this time.

We first undertook to measure the adhesion by a shearing method. The samples were made up into triplets with a carefully prepared cement mortar. The cement used was the Atlas Brand having a tensile strength neat of 600 lbs. per sq. in.

The sand was a sharp bar sand screened to pass a 20-mesh sieve. In laying the triplets the bricks were plunged into water and laid up as nearly vertical over one another as possible. As soon as set sufficiently to be handled, they were stored in a basement room to season. They were then placed on the testing machine and the amount of force necessary to shear the center one from between the outside ones was determined.

Great care was exercised in shoring up the specimens by means of thin metal wedges in order to get as nearly as possible a true shearing stress.

Five prepared specimens of each sample were tested and the results reported are the average values. There was a considerable variation in the values obtained from samples but on the whole the agreement between the different specimens was as good as could be expected from this kind of test and a few of them were remarkably good.

The values obtained are arranged in Table VI in the order of greatest absorption by the total immersion method.

They can be compared by reference to Fig. 3 in which they have been plotted using the absorption percentage and shearing stress per square inch as coördinates.

They divide themselves into three groups. The center group contains 12 out of 23 which are, with the exception of No. 23, building brick ranging from soft salmon to hard arch. No. 23 is a vitrified sewer brick with smooth surface and the rather high

Table VI.—Comparison of Absorptive Capacity and Adhesion of Mortar.

Sample No.	Kind of Brick.	Absorption by Total Immersion 17 Days. Per cent.	Shearing Stress. Lbs. per sq. in.	
			Mortar 2:1, 21 Days.	Mortar 4:1, 14 Days.
3	Salmon	12.83	187	42
13	Repressed	10.84	142	58
4	Repressed	10.66	132	93
2	Red building	10.60	115	35
20	Red building, shade 4	9.68	130	69
6	Arch	9.25	143	56
21	Red building, shade 10	9.16	184	41
1	Arch	8.83	93	25
14	Paving block	8.49	183	67
5	Machine, red	8.22	174	61
8	Hard arch	7.89	98	36
9	Hard arch	7.27	111	43
17	Red building	6.90	172	56
7	Black heads	6.89	58	27
18	Pressed, shade 79	6.78	145	55
23	Vitrified sewer	6.59	68	74
19	Pressed, shade 149	5.83	93	46
16	Black heads	5.48	199	58
22	Paving block	3.14	96	56
15	Arch	2.16	89	38
12	Wire cut, shade 60	1.39	137	..
11	Repressed, shade 12	0.70	91	29
10	Repressed, shade 12	0.68	79	23

Note.—Each value is the average of five tests.

absorption of 6.59 per cent. The group on the left consists of five and that in the upper part of five, with No. 16 standing considerably removed.

Considered alone from the standpoint of absorption it would seem rather difficult to draw any very reliable conclusions from the distribution. There are, however, other variables affecting the results, the most potent of these being difference in composition and difference in surface.

The variations in composition affect the chemical reactions which might take place at the surface of contact. We have as yet no data upon the chemical composition and must therefore omit it from the consideration.

The surface conditions, however, would be important in the kind of test which we have made. Let us consider them in the case of the two smaller groups. No. 10 is a recessed, repressed

wire cut brick, Nos. 11, 12 and 15 are wire cut and very rough, and No. 22 is a cleated, grooved paving block with rough surface. These are all conditions which tend to increase the adhesion as determined by a shearing method. In the upper group, No. 5 is a red machine building brick, with a peculiar smooth but porous

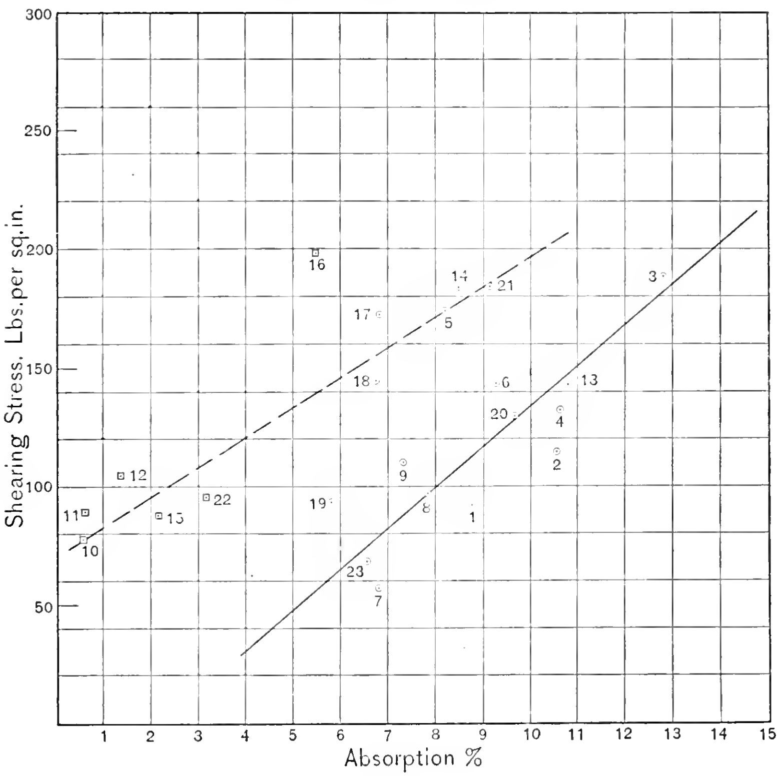


FIG. 3.—Relation between adhesion and absorptive capacity.

surface. No. 14 is a paving block with fairly smooth surface. No. 16 is a wire cut black head. No. 17 is a wire cut building. Nos. 18 and 21 are pressed brick with small rough spots, about one to each square inch but, except in this particular, identical with Nos. 19 and 20.

Nos. 18 and 21 are the only ones in this group in which the surface condition would not tend to increase the shearing stress.

It may be a coincidence, but a line drawn as a fair average through the two smaller groups is nearly parallel to an average drawn through the larger group. Any condition which would tend alone to lower the shearing stress would bring the two lines more and more nearly into coincidence.

We therefore feel that the results are sufficiently harmonious to justify us in extending the investigation to other specimens of the same samples, with their surfaces ground similar by some standard abrasing material.

We may be able in a measure to eliminate the effect of surface, by tests which we intend to make by direct tension upon the two bricks of each prepared specimen which has not been destroyed in the shearing test.

At any rate the test shows the adhesion of the mortar to these particular samples in the exact condition in which they would be used.

We wish to express our thanks to the following firms who have coöperated with us by generously supplying the samples for the work:—The New Washington Brick Co., West Bros., Potomac Brick Co., O. W. Ketcham representing the Kittanning Brick and Fire Clay Co., The Frederick Brick Co., The Hydraulic Press Brick Co. and The Engineer Commissioner of the District of Columbia.

DISCUSSION.

Mr. W. K. Hatt.—It appears to me that the explanation of the increase of adhesion of the mortar in case of the bricks showing greater absorption may be very simple. The more absorptive bricks take the water more rapidly from the mortar, which dries out more quickly and becomes stronger at an early period. **Mr. Hatt.**

Mr. D. E. Douty.—I do not attempt an explanation, but simply present the results which we have obtained. It is, of course, a fact that a brick which has a high absorption would absorb the water from the mortar more readily, and it might be that in a short-time test, such as twenty-one days in which we made this test, the mortar had hardened more in the more absorbent bricks than it did in the others. I failed to mention in the paper that we have a set of specimens, made at the same time, with the same mortar, which we are aging to see if time will make any difference in the shearing stress. We shall probably allow them to stand for four months. **Mr. Douty.**

The President.—What was the nature of the failure in shearing? Did the mortar cleave from the brick or did the brick shear? **The President.**

Mr. Douty.—In most cases the mortar cleaved partly from one brick and partly from the other. In very few cases did we succeed in shearing the center brick completely out from between the outside ones. We had difficulty with the soft bricks which are represented here by the first five specimens, because the adhesion of the mortar to the bricks was greater than the cohesion of the brick, and hence failure often occurred in the body of the brick rather than at the surfaces of contact. **Mr. Douty.**

Mr. I. H. Woolson.—I should like to ask if in bedding the brick plaster of Paris was tried. My experience has been that in the pressure test of bricks it is necessary to have them well bedded. Although I have made a large number of tests using blotting paper and other things, I am more and more convinced that in these tests the surfaces ought to be bedded in plaster of Paris. **Mr. Woolson.**

Mr. Douty. MR. DOUTY.—Yes, we tried plaster of Paris with the result, that when we made the plaster of Paris sufficiently thick to work on the machine at all well, the moment the brick touched it, it set quickly in the center—leaving us a condition over our parallel something like this. (Illustrating.) If this is a section of the plane of the parallel, the plaster of Paris would form in a little mound like that, thin at the edge. In order to prevent that it would be necessary to make the plaster of Paris very thin. We worked at it for some time and broke quite a number by that method, but in the softer brick, the bricks themselves broke. It was evident, although we proceeded very carefully, that we could not get a good bearing surface right up to the edge of the brick on the inside. We finally decided to shore up with very thin metal wedges, putting them in every place where there was doubt about the bearing.

Mr. Woolson. MR. WOOLSON.—May I make a suggestion? I have had considerable experience in that line, and I have found that with brick or other material, that would be weakened by the absorption of moisture from a facing of plaster of Paris, the difficulty may be entirely overcome by first giving the surface a coat of shellac put on with a brush. It will not affect the strength, and the plaster will adhere to it perfectly.

Mr. Douty. MR. DOUTY.—That is a very good suggestion. The using of any material to coat the bricks to prevent their absorption has not occurred to us, and I think it would work very well. I am pleased to have any further suggestions, because this is only the beginning. We expect to carry the investigation further. Those who have had experience with practical tests know that the variables in this kind of a test are numerous and the results obtained by different investigators are often discordant.

Mr. Humphrey. MR. R. L. HUMPHREY.—Mr. Chairman it seems to me that there are a number of points in this test that are subject to criticism. We have had considerable experience in St. Louis in making shearing tests of concrete. We find that it is extremely difficult to devise any test which will give true shear. The test which we apply, which is similar to that which Mr. Douty used, developed tension in the lower, and compression in the upper fibers, although the resultant failure was probably due to shear. It appears to me that unless the load is applied parallel to the face of the brick that

there will be a bending action which would cause the joints to open at the bottom and thus produce failure. Mr. Douty has stated that very rarely did the center brick shear, but that the failure was at one or the other of the joints, starting at the bottom. In any event it would seem to me that the two bricks should be supported and the center brick sheared out in order to reduce the bending to a minimum. **Mr. Humphrey.**

It would also seem to me that the test is defective in that the behavior of the bricks would be largely due to the strength of the mortar in the joints and I should like to ask Mr. Douty whether any effort was made to obtain any degree of regularity in the thickness of the joints as the thickness of the mortar in the joints would necessarily have a material influence on the result of the tests. The different degrees of absorption in the bricks would also have an important influence on the results of the tests and it would seem to be proper to thoroughly saturate the bricks first so that this influence might be reduced to a minimum. The test reminds me somewhat of a condition which prevailed many years ago in the City of Philadelphia. It was the practice of the inspector on the work to test the quality of a cement by sticking two bricks together with a joint of the cement and that cement was adjudged the best which would hold the bricks together in the shortest interval of time. Under such a test the quick-setting natural cement would prove superior to the slow-setting Portland cement. This would seem to be a parallel case and it is probable that the difference in strength is due to the absorption of the brick which accelerated the setting of the cement.

MR. DOUTY.—I should like to say in this connection, that we have four surfaces; and the variation in getting contact, that is, getting the same contact on the four surfaces, would probably be greater than the variation of which Mr. Humphrey speaks. In building up a triplet there are four surfaces for shearing the bricks from the mortar, and, of course, it would be practically impossible to get all of those four surfaces exactly the same. So it occurs to me that although the testing conditions were perfect, it would be a very rare instance in which you would get complete shearing-out of the center brick. The fact that the mortar does not always fracture upon both sides is no indication that it was not a true shear. **Mr. Douty.**

Mr. Douty.

As to the regularity between the thickness of the mortar joints, I would state that there was no effort to secure it, except by the eye. We simply tried to make them uniform. Perhaps the variation in the thickness might affect the results somewhat, but the compressive strength of the mortar is so much greater than the strength of the brick that we did not think that the slight variation that we would get in the thickness would affect our results materially, since the shear should take place at the surface of separation.

Mr. Brown.

MR. J. G. BROWN.—I should like to ask if the difference in strength is not governed more by the character of the surface of the brick than its absorptive properties. For instance, repressed bricks have very low adhesive strength to mortar, which would lead me to believe that the surface of such bricks is probably more glassy than that of soft bricks. The same thing is true in concrete, and we know also that it is very difficult to get cement mortar to adhere to glass unless the surface has been roughened.

Mr. Douty.

MR. DOUTY.—In reply I would say that we are convinced that the surface condition is very important, and the next step of the investigation will be to take a specimen from the same samples and grind the surface with some standard abrading material and attempt as far as we can to eliminate surface irregularities. We will then, of course, not get surfaces just alike, because the difference in porosity will make some difference in the surface. We hope to eliminate the surface condition also by taking the remaining pairs which are still unseparated and breaking them by direct tension.

FOREST SERVICE TESTS TO DETERMINE THE INFLUENCE OF DIFFERENT METHODS AND RATES OF LOADING ON THE STRENGTH AND STIFFNESS OF TIMBER.

By McGarvey Cline.

The experiments of the Forest Service to determine the influence of different methods and rates of loading on the strength and stiffness of timber may be divided into the following classes:

1. Tests to determine the relation between the strength functions and the different rates of loading procurable in an ordinary universal testing machine.

2. Tests to determine the strength of wood under the continued application of a dead load.

3. Tests to determine the resistance of wood to impact.

The purpose of the first class of experiments was to establish relations which would make possible the correlation of results secured from tests made with the different speeds of machine ordinarily encountered in general laboratory work. These experiments were carried on at the Forest Service laboratory at the Yale Forest School under the direction of Mr. H. D. Tiemann whose discussion of the results secured appears in another paper.

The other two classes of experiments are for the purpose of establishing the relation of the strength values developed under dead and impact loading to the values secured from the standard methods of testing under gradually applied loads. These experiments are still in progress, and it is the purpose of this paper to briefly describe the apparatus used in connection with this work.

Apparatus for Making Tests Under Dead Loading.

The apparatus for making dead-load tests consists of the following parts:

1. *A System of Levers for Applying the Load.*—Fig. 1 shows these levers in position beneath the floor of the laboratory at the Yale Forest School. They are provided with knife-edge fulcrums

and have a multiplying ratio of 5 to 1. The load is transmitted to the test specimen by means of the rods and turn buckles shown in the right-hand portion of the figure. As the test specimen deflects the levers are kept in a horizontal position by means of the turn buckles. The load is applied by placing cast-iron weights on the levers as indicated in the left-hand portion of the figure. The method of applying the load to the specimen is shown in Fig. 2.

2. *A Device for Measuring and Magnifying Deflections.*—This device is shown in Fig. 2. It is constructed of sheet metal

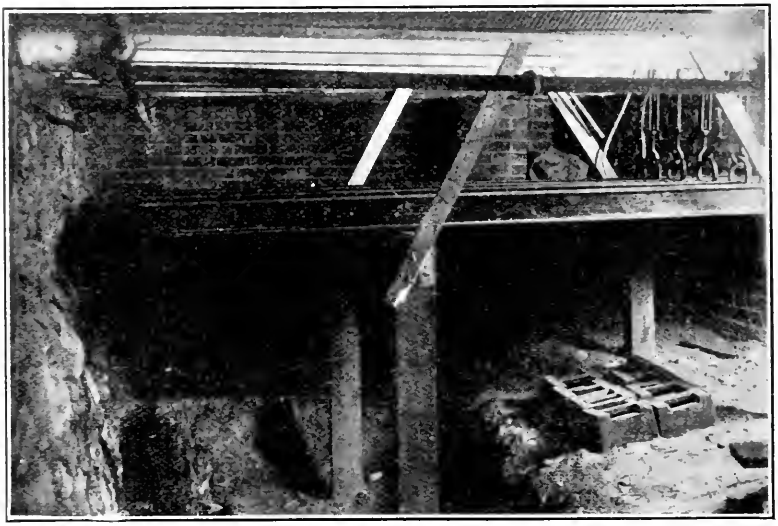


FIG. 1.—Dead Load Apparatus, showing loading levers.

and is supported by small nails driven in the neutral axis of the test specimen. The deflection is taken from the neutral axis of the specimen by means of a wire yoke and a flexible wire which passes over the small pulley vertically above the center of the test specimen to the hub of the larger pulley shown in the right of the figure. The diameter of the larger pulley is five times that of its hub, thus multiplying the deflections by 5. From the larger pulley the fine iron wire runs to the recording drum.

3. *The Recording Drum.*—The recording drum (Fig. 3) is driven at a uniform velocity by means of clockwork just beneath the top of the drum case. The fine wires for recording the deflections

pass from each test specimen over suitably adjusted pulleys to the markers. The markers are weighted and move freely along the vertical rods composing the frame of the drum case. These rods are kept polished and oiled so that the wires transmitting the deflections will be kept under uniform tension. The whole drum mechanism is enclosed in a dust-proof glass case. Fig. 4 shows the apparatus in operation, the levers being beneath the floor of the laboratory.

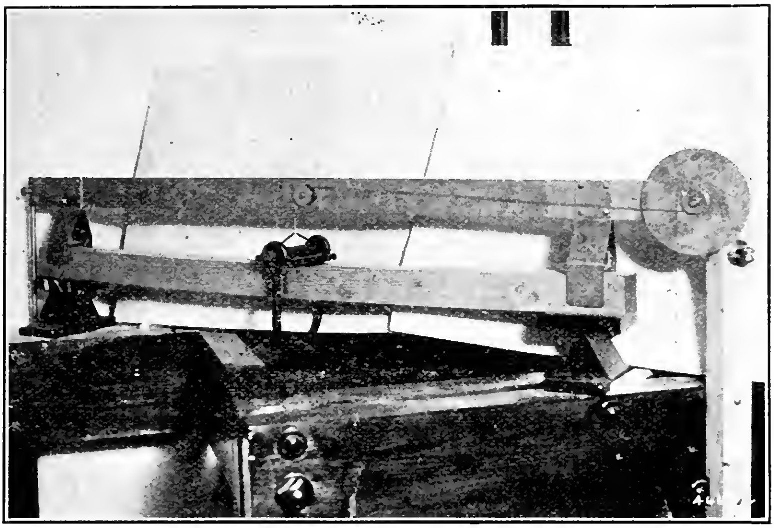


FIG. 2.—Dead Load Apparatus, showing method of applying load to specimen and measuring deflections.

IMPACT TESTING MACHINE.

The impact tests of the Forest Service have, thus far, been made on the machine and according to the methods described in a paper by Dr. W. K. Hatt and Mr. W. P. Turner, appearing on page 462, Vol. VI, of the Proceedings of this Society. This method of testing has been very useful in determining the relative brittleness of different materials, and the results secured indicate that wood has much higher elastic strength under impact loads than under dead or gradually applied loads. This machine, however, is not well adapted for investigating the behavior of material under repetitive impact loading, and it is difficult to correlate data secured

from it with the data being secured from tests made under gradually applied and dead loads. To overcome these difficulties a new impact machine, designed by the writer and Rolf Thelen, will be erected by the University of Washington, Seattle, Wash., for the Forest Service laboratory run in coöperation with that institution.

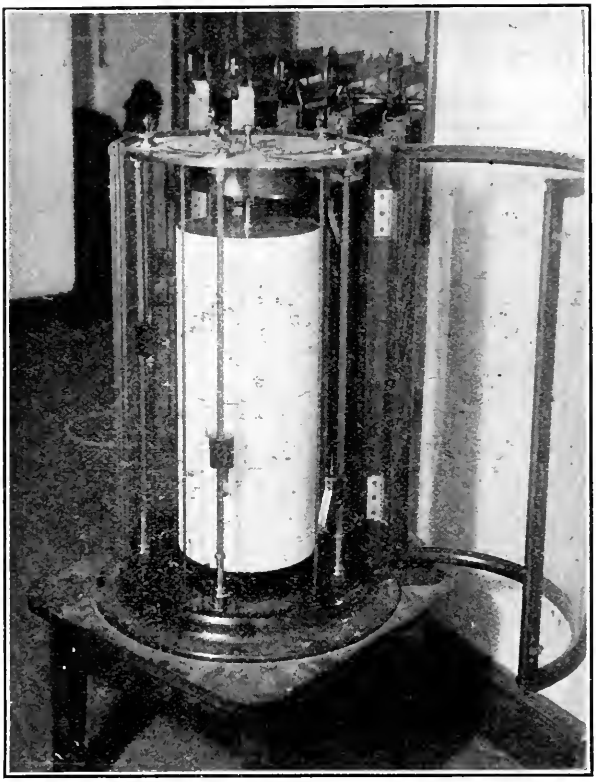


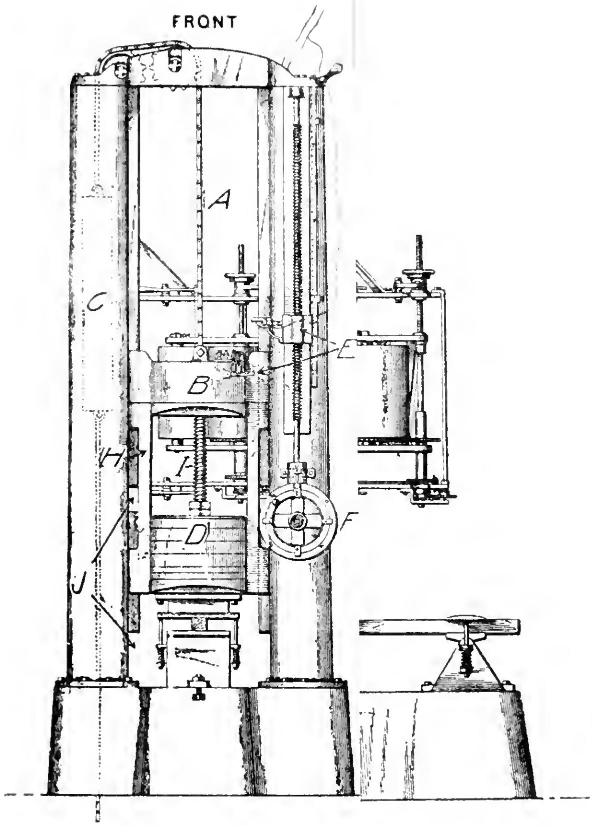
FIG. 3.—Dead Load Apparatus, showing method of reading deflections on reading drum.

FOREST SERVICE AUTOMATIC AND AUTOGRAPHIC IMPACT TESTING MACHINE.

Plate IX shows an assembled drawing of the machine.

Base and Frame Work.—The cast-iron base of the machine on the upper face is 6 ft. long. At the base of the columns it is $45\frac{1}{2}$ ins. wide, and 16 ins. wide at the ends. The base is 17 ins. deep

PLATE IX.
PROC. AM. SOC. TEST. MATS.
VOLUME VIII.
CLINE ON FOREST SERVICE TESTS.



AGRICULTURE

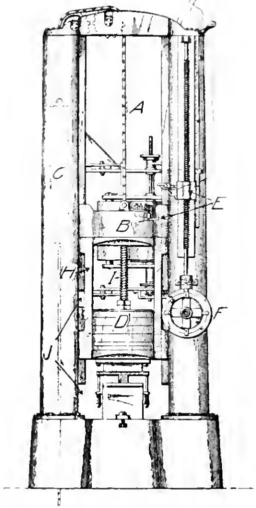
ERVICE

C IMPACT TESTING MACHINE

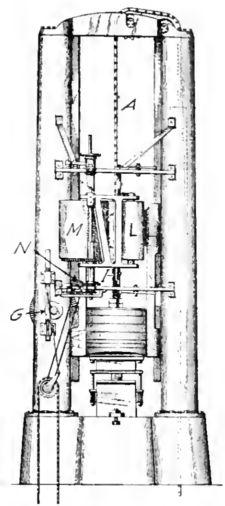
line and Rolf Thelen

2 Feet

FRONT



BACK



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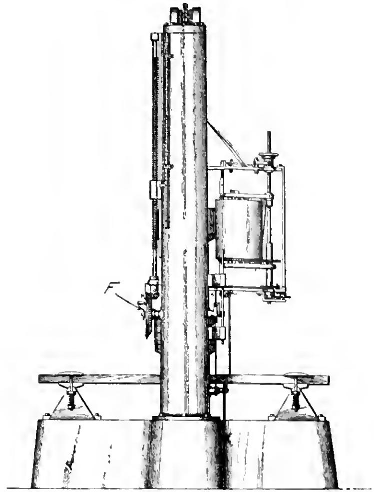
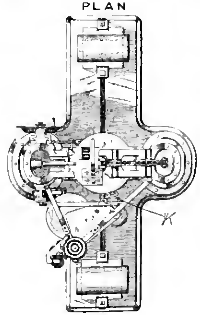


PLAN



U. S. DEPT OF AGRICULTURE
FOREST SERVICE
AUTOMATIC AND AUTOGRAPHIC IMPACT TESTING MACHINE

Designed by McGarvey Cline and Rolf Thelen

Scale
0 1 2 Feet

May 1908

and weighs, approximately, 7,500 lbs. It will be mounted on a massive concrete foundation. The distance from the base to the top of the columns is 8 ft. and the clearance between the columns is 19 ins.

Motive Power.—The machine is driven by an electric motor operating through a worm a crank having a 3-ft. throw. To the crank is fastened the roller chain A, and as the crank revolves the magnet lift B is given a reciprocating motion.

Magnet Lift and Tripping Mechanism.—The hammer D is raised by means of the magnet B which is partially counter-

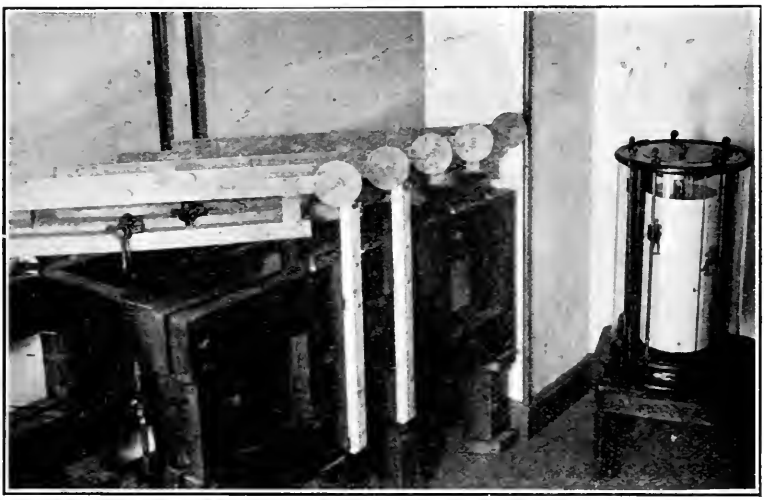


FIG. 4.—Dead Load Apparatus, showing general view of apparatus in operation.

weighted by the weight C. The hammer is released at the desired point by breaking the circuit in the magnet lift. This is accomplished by the trip E. When the part of the trip mounted on the magnet comes in contact with the part mounted on the right-hand column the closing of an electric circuit throws a switch which breaks and reverses the current in the magnet lift, thus causing a quick release of the hammer. This switch may be seen in the plan. The height of the trip on the column is adjusted by means of the hand wheel F which, if desired, may be adjusted automatically by means of the mechanism G.

The Hammer.—The hammer consists of a cast-steel frame H and a number of cast-iron discs which are clamped in position by nuts operating on the heavy screw I. The frame work of the hammer weighs 500 lbs. and each disc 100 lbs.; when all of the discs are in use the weight of the hammer is 1,500 lbs. The striking face of the hammer is provided with suitable heads for transmitting the energy to the center of the specimen under test. The hammer may be removed from the machine by lowering it until it rests upon the base; the guides on the hammer then come opposite the slots J in the column guides and the hammer may be taken out of the machine on rollers.

Recording Mechanism.—The recording pencil K is fastened to the frame of the hammer and bears upon a strip of paper which is wound automatically from the drum L to the drum M, the drum M being operated by the mechanism N. The recording mechanism provides for the vertical adjustment of the recording drums and also for complete control of the recording pencil.

In all investigations to determine the influence of different methods and rates of loading, specimens approximately 2 x 2 ins. in cross-section and 30 ins. long are used. Experience has shown that the laws deduced from tests on such specimens apply in a general way to the sizes used in actual structures. It is the purpose, however, of the Forest Service to check the investigation on small specimens by a limited number of tests on full-sized materials such as bridge stringers, car sills, etc.

THE EFFECT OF THE SPEED OF TESTING UPON THE STRENGTH OF WOOD AND THE STANDARDIZATION OF TESTS FOR SPEED.

By Harry D. Tiemann.

In considering the effect of the speed at which the stress is applied in testing materials, the subject naturally divides itself into three distinct methods of operation, namely: (1) testing by impact, (2) testing at uniform speed, as in ordinary machine tests, and (3) testing under constant or "dead" load.

Mr. Cline has shown in the preceding paper the character and the importance of the work being done in this field, by the United States Forest Service. It is my desire to present here more specifically the conclusions and the results which we have derived pertaining to the second method of operation, namely, testing wood at different speeds upon the ordinary testing machines, viewing the subject both from the theoretical standpoint and from that of its application to the standardizing of tests.

That wood has a greater resisting power to rapidly applied stresses of short duration, than to loads applied gradually or remaining upon the material for a longer time, is a well-known fact, but little appears to be known of the fundamental laws underlying the relations of time and strength. It has been the object of this research, which has been conducted by the writer at the Yale Laboratory of the Forest Service, to determine these laws.

Work of Other Investigators.

Reviewing first such work as has been done along this line, mention may be made of a few tests made by Professor R. H. Thurston, upon small "yellow" pine beams, 1 x 1 x 40 ins., one of which broke with 60 per cent. of the normal load after fifteen months under stress. (See Thurston's "Materials of Engineering," Part 2.)

At the Massachusetts Institute of Technology, Professor Lanza has been carrying on some "dead load" tests upon eight large longleaf pine beams (about 6 x 12 ins. and 20-ft. span) which

are reported to be still deflecting, having been loaded with an average center load of 6,500 lbs. since March, 1904. In his Applied Mechanics, page 689, are given the detailed results of some twenty-four time tests upon large beams with 20-ft. span and the comparison of the results with similar material tested in the ordinary way. The beams were loaded for six months, more or less, and finally tested to rupture. These results show that the modulus of elasticity after the long period of stress, was reduced to about one-half of the original value. The final modulus of rupture however, when the beams were tested in the usual manner showed no decided difference. These beams were not stressed above from 1,000 to 1,700 lbs. per sq. in. maximum fiber stress.

J. B. Johnson has made about seventy-five tests in crushing endwise upon longleaf pine specimens about $1\frac{1}{2}$ ins. square and 3 ins. long, thoroughly seasoned, the results of which are described and a time-strength curve given in his Materials of Construction, page 468a. From this he concludes that "the ultimate strength of columns under permanent loads is only about one-half of the ultimate strength of the same columns as determined by actual tests in a testing machine." He further concludes that the reasoning applies to other forms of stress in wood.

Wood under suddenly applied loads, on the other hand, offers greater resistance than it would in an ordinary test. Thus wood subjected to impact tests shows in some cases about double the elastic strength values of the ordinary machine test.

Certain metals, as Thurston has shown (American Society of Civil Engineers, 1876 p. 199, 1877 p. 28), exhibit a similar peculiarity while others do not. There appears to be a condition of semi-fluidity in such metals, so that the particles gradually flow under constant stress. Tin, copper, lead, etc., show this characteristic, while iron, steel, etc., exhibit no change due to time.

It is evident, therefore, that the influence of time must always be taken into account in testing wood.

SOME RESULTS OF DEAD LOAD TESTS.

Thus far, some results of dead load tests made upon the special recording deflectometer designed by the writer, which Mr. Cline has just described, have been obtained. Other tests are in progress at the present time. While it is premature to generalize very much from these few experiments, two deductions are worth noting here.

1. The results of tests on longleaf pine beams show the same peculiarities noted by Professor Lanza, namely, that when the beams were not ruptured by the dead load, and were subsequently tested in the usual testing machine, after resting for a year without load, the ultimate strength had not been reduced, although the modulus of elasticity showed a weakening of about half.

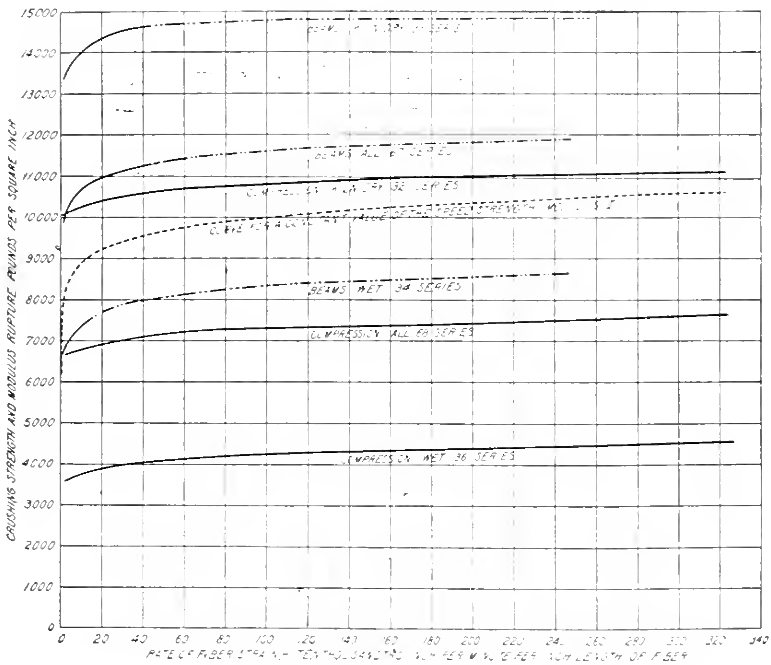


FIG. 1.

2. The variations which occur in the moisture of the normal atmosphere have a decided influence on the deflections under dead load. The beams deflect most during damp weather, and this deflection is not recovered by subsequent drying.

SPEED-STRENGTH TESTS.

Theoretical Considerations.—In the usual method of making tests the cross-head of the testing machine is made to move, as nearly as may be, at a uniform speed, independent of the stress

being developed in the material. It is evident, therefore, that the *rate of strain* and not the *rate of stress* should be used as the basis for our deductions. The rate of strain being simply kinematical, can be controlled, whereas the rate of stress is circumstantial and can not be determined in advance. For a given speed of the cross-head, the rate of fiber strain will depend upon the size of the specimen; thus a compression specimen 6 ins. long would have double the rate of strain to which one 12 ins. long is subjected. Hence, the 6-in. specimen would apparently show greater strength than the 12-in. one.

If the speed of the cross-head in inches per minute be expressed by n, then for compression or tension the rate of fiber strain is simply

$$z = \frac{n}{l}$$

when l is the length of specimen. For a beam supported at the ends and loaded at the middle, when h is the depth and l the span, the rate of fiber strain (of extreme fiber is)

$$\frac{6h}{l^2} n$$

and for third point loading,

$$\frac{4.7h}{l^2} n.$$

If the results of tests upon the same material be plotted on cross-section paper, with fiber stress as ordinates, and rate of fiber strain or speed of cross-head as abscissas, convex curves will be obtained similar to those shown in Fig. 1.

Such diagrams, however, are misleading since they do not convey to the mind the true proportionality of the quantities. The element of *time* rather than the amount of *space* the cross-head has moved in a unit of time, impresses itself upon the mind in reasoning out such problems. This form of curve, however, in which space is the abscissa, shows great differences for minute periods of time, and extremely small spaces for long periods. Moreover, for the slow tests covering several hours the speed can be more accurately measured and controlled than for the fast tests requiring but a few seconds. So these curves plotted with rate of fiber strain for abscissas give significance to these quantities in just the reverse order of truth.

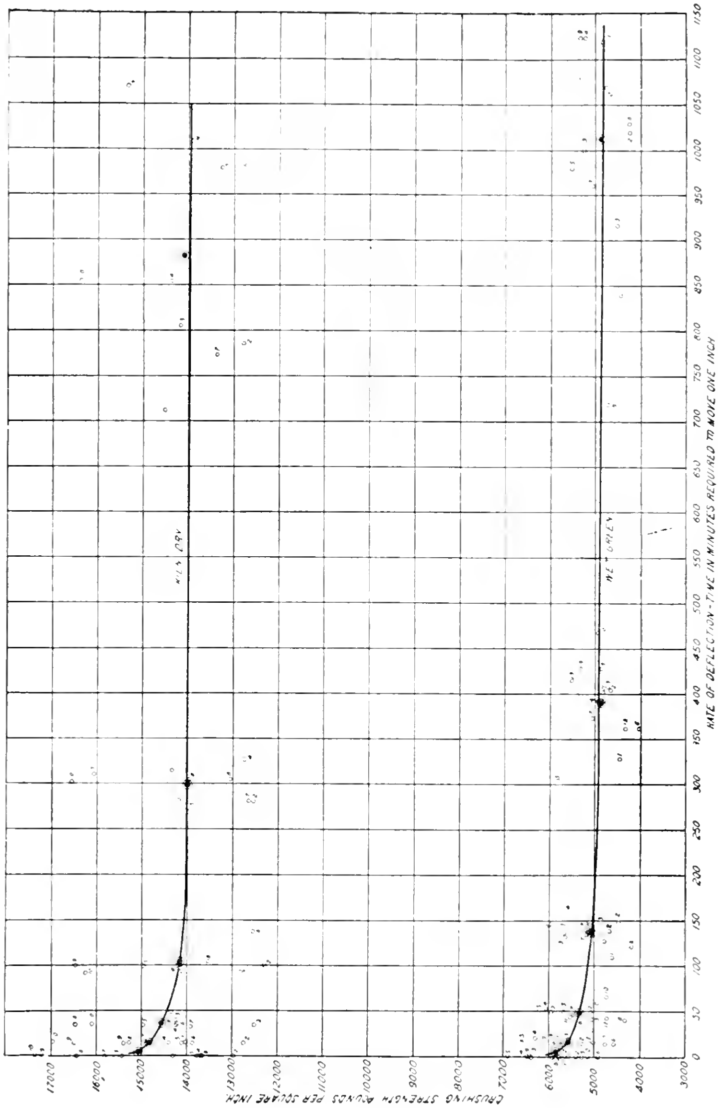

KILN DRY
RATE OF DEFLECTION-TIME IN MINUTES REQUIRED TO MOVE ONE INCH
CRUSHING STRENGTH POUNDS PER SQUARE INCH

Fig. 2.

By using the reciprocal of speed for the abscissas, which is proportional to the *time* required for the cross-head to move one unit of space, these same curves become reversed and concave, as shown in Fig. 2. Here the fast speeds are shown by very small distances and the slow speeds by great distances, which comports better with our natural conceptions.

Either of these forms of curve could be used for reducing the results of tests to the equivalent values at standard rate of fiber strain, but neither form exhibits satisfactorily the fundamental law of speed and strength, because they are both circumstantial, the values used being arbitrary definitions: "pounds per square inch" and "inches per minute" or "minutes per inch." An answer to the question "At what speed should the test be made to give the smallest change in load for a given change in speed?" is by no means as self-evident from these curves as it at first appears. Looking at the first form, Fig. 1, one might naturally say the faster the speed the less would be the relative change in strength, and looking at the other form, Fig. 2, directly the opposite conclusion might be drawn. The apparent contradiction lies in our definition of what is meant by "a given change in speed." This expression might mean an arithmetical increment of either space (so many inches per unit of time) or of time (so many minutes per inch) or it might mean by a proportional change in speed, say by 10 per cent. of the given values, for example. Further thought will show, that it is the *proportional change in speed* which should be thought of in making tests. So, likewise, it is the *proportional change in strength* which should be used rather than the numerical value. By using these ratios for the comparison of speed and strength we obtain the absolute relation independent of circumstantial or arbitrary figures.

Calling the strength L and the speed V, we therefore obtain the expression

$$T = \frac{dL}{L} \times \frac{V}{dV}$$

as representing the fundamental law between speed and strength. This I have designated the "Speed-Strength Modulus," which is the ratio of the proportional change in strength to the corresponding proportional change of speed. It will be observed that the same value for T is given by either of the two forms of curves, which therefore reconciles the seemingly contradictory results.

The speed-strength modulus is a coefficient which, if the proportional change in speed be multiplied by it, will give the proportional change in strength, at any given speed. For example, how much would a change of 50 per cent. in speed affect the strength of wet wood in bending, if running at a rate of fiber strain of 0.0018? In Table II the modulus is given for these conditions as 0.053; therefore the strength would be affected by 50 x 0.053 = 2.65 per cent.

Now

$$\frac{dL}{L} = d \log L \text{ and } \frac{dV}{V} = d \log V.$$

If, therefore, we draw a curve in which the ordinates are

$$x = \log V,$$

and the abscissas

$$y = \log L,$$

the slope of its tangent at any point will be the speed-strength modulus, for this slope is

$$\frac{dy}{dx} = \frac{d \log L}{d \log V} = \frac{dL}{L} \times \frac{V}{dV} = T.$$

This curve is independent of the numerical values used for L and V, and will give the same value for T whether

$$x = \log V \text{ or } x = \log\left(\frac{1}{V}\right).$$

Fig. 3 gives the results of the tests expressed by logarithms in this way. Evidently the steeper the curve, the greater is the value of the modulus T and therefore the greater the proportional variation in strength for the corresponding proportional variation in speed.

By drawing upon this same plate rectangular diagrams whose diagonals represent by their steepness various percentage variations of strength for given percentage variations in speed, a convenient means is obtained for determining these relations at any point of the curve. All that is necessary is to draw a line in this diagram through its origin parallel to the tangent of the curve at the given point and note where it intersects the vertical and horizontal strength and speed lines. This diagram will be found on the plate, its location being purely arbitrary. The converse

of this operation is equally simple. For example, for 50 per cent. variation in speed, what point on the curve will give a change of one per cent. in the strength? Select the diagonal in the diagram which passes through the point for 50 per cent. speed and one per cent. load and draw a tangent to the curve parallel to the diagonal. The point of tangency, then, gives the logarithm of both the speed (rate of fiber strain expressed in ten-thousandth inch per minute per inch of length of fiber) and of the strength (in pounds per square inch) which fulfil the conditions.

The reducing or standardizing factor for reducing the strength value obtained at one speed to its equivalent at a standard or any other speed is more readily obtained by use of the speed-strength form, as in Fig. 1, although it may be obtained from the logarithmic curves, Fig. 3.

Evidently a straight line on the logarithmic plate would signify a constant relation between speed and strength, and if it were horizontal it would mean constant strength for all speeds.

The relationship between the logarithmic and the speed-strength curves is readily shown by studying the significance of the constants a and b in the equation of a straight line on the logarithmic curves

$$y = ax + b, \ \log L = a \log V + b,$$

which gives for the speed-strength curves

$$\frac{L}{V^a} = b \text{ or } L = bV^a.$$

Description of the Tests.—Tests were made upon beams 2 x 2 ins. in section with a span of 36 ins. and the load applied at the center, and upon compression specimens of the same size section and $5\frac{3}{4}$ ins. long, the stress being applied parallel to the grain. The method of operation except for the speeds was the same as in the "Moisture Strength" research described in my paper in the Proceedings of last year, and more fully in Bulletin 70 of the Forest Service. The tests were made upon a 30,000-lb. four-screw Olsen machine which was geared by a countershaft to run at eight different speeds, numbered from 1, the fastest, to 8, the slowest. Each successive speed was intended to be about one-third that of the preceding, but owing to stretching of belts and slippage, the ratio was only approximate. It was found that for the dry

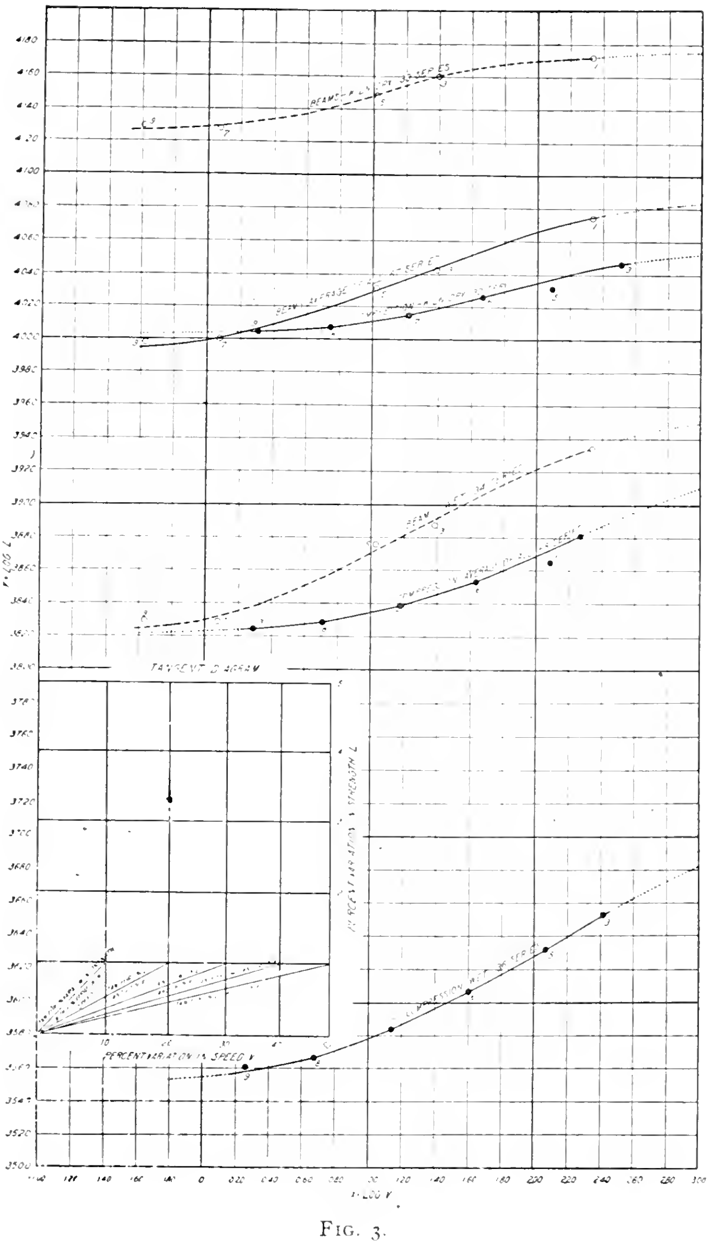

BEAMS—KILN DRY 33 SERIES
TANGENT DIAGRAM
PERCENT VARIATION IN SPEED V
COMPRESSION WET 36 SERIES
LOG V
LOG L

FIG. 3.

compression pieces, which required a heavy load, the corresponding speeds were lower than for the wet ones, and still less than for the beams. The cause was probably the same as just stated. The machine was operated by a gas engine, to which it was belted through three countershafts. A still lower speed, No. 9, was obtained by operating the machine by hand.

Range of Speeds.—For compression tests the lowest speed amounted to about 0.001 in. per min., and for beams, 0.004 in. per min., so that from one-half to two hours were required to make a compression test, and over six hours for some of the beam tests. This gave a range in speeds of 1 to 150 for compression, and 1 to 500 for beams.

Table I gives the approximate speeds and their corresponding number. The usual tests at this laboratory are made at speed No. 7 for compression, and speed No. 5 for beams.

TABLE I.—APPROXIMATE SPEEDS AT WHICH THE TESTS WERE RUN.

No. of Speed.	Compression Tests.		Flexure Tests.	
	Approximate Speed of Cross-head. Ins. per min.	Rate of Fiber Strain, per minute.	Approximate Speed of Cross-head. Ins. per min.	Rate of Fiber Strain, per minute.
1	......	......	2.3	0.0213
2	......	......	......	......
3	0.16	.0278	0.26	0.0024
4	......	......	......	......
5	0.069	.0120	0.113	0.0010
6	0.024	.0042	......	......
7	0.0085	.0015	0.013	0.0001
8	0.0029	.0005	......	......
*9	0.0011	.0002	0.0045	0.00004

Material Used.—Tests were made from 1904 to 1906 upon three species, longleaf pine, red spruce, and chestnut, both green and kiln dry, thus making twelve groups of tests. A "series" consisted of several specimens cut from the same plank or strip and as similar in structure as possible, each specimen in a series to be tested at a different speed. Each group contained about ten such series of specimens. Each series of beams contained five specimens and each series of compression specimens, six.

* Intermittently turned every minute.

It was found impracticable to run more than five speeds for the beams owing to the difficulty of selecting more than five specimens of uniform quality for each series. In the compression tests speed No. 3 was the fastest speed which was practicable on account of mechanical difficulties. Many of these tests required but twenty seconds.

It is reasonable to suppose that for very rapid speeds, the inertia of the parts of the machine would exert an influence and need to be considered. The machine was, therefore, calibrated for this effect, which was found to be entirely negligible at any of the speeds used.

Results.—The separate test values were plotted for each group and an average curve drawn as shown in Fig. 2. Then for each kind of test the average of the three species in the green condition was made, and in the kiln-dry condition, and also the average of all, wet and dry. These average curves are given on Figs. 1 and 3, the actual average points with the speed numbers being shown on Fig. 3.

From these curves it will be seen that the variation of strength is noteworthy, and increases with the speed. Table II gives the relative strengths at various speeds chosen arbitrarily at ratios 1 : 3, and also the value of T, the speed-strength modulus. For clearness only the ultimate strengths in bending and in compression are entered on the curves. The modulus of elasticity shows very little change in the beam tests but nearly as much as the ultimate strength in the compression tests. The stress at elastic limit on the other hand shows very little change in the compression tests but in the beam tests it changes as much as the ultimate strength in compression. The last fact is what would be expected when one remembers that the elastic limit in beams corresponds very closely to the ultimate crushing strength of the material.

If the law of proportionality of dimensions to the number of vibrations or to the pitch of the sound emitted by a piece of wood be strictly true, it follows that the modulus of elasticity must necessarily be constant at all speeds. Such experiments would be difficult to make with accuracy owing to the heterogeneity of the substance, but it would be interesting to know if this law holds true in the case of wood.

It will be noticed that the wet specimens invariably show a

TABLE II.—SPEED-STRENGTH MODULI, AND RELATIVE INCREASE IN STRENGTH AT RATES OF FIBER STRAIN INCREASING IN GEOMETRICAL RATIO.

(From average curves, Fig. 3. Values in parentheses are approximate.)

	Rate of Fiber Strain. Ten-thousands in. per min. per in.	2/3			2			6			18			54			162			486		
COMPRESSION.	Speed of cross-head, Ins. per min.	0.000383			0.00115			0.00345			0.0103			0.0310			0.0931			0.270		
	Specimens	Wet	Dry	All	Wet	Dry	All	Wet	Dry	All	Wet	Dry	All	Wet	Dry	All	Wet	Dry	All	Wet	Dry	All
	Relative crushing strength	....	....	...	100.0	100.0	100.0	103.4	00.8	101.5	107.5	102.7	103.8	113.9	105.5	107.9	121.3	108.3	116.4	128.8	110.0	118.9
	Speed-strength modulus, T′	....	....	...	0.017	(0.006)	(0.009)	0.033	0.012	0.016	0.047	0.021	0.029	0.053	0.027	0.039	0.060	0.023	0.049	(0.052)	(0.015)	(0.040)
BENDING.	Speed of cross-head, Ins. per min.	0.0072			0.0216			0.0648			0.194			0.583			1.75			5.25		
	Specimens	Wet	Dry	All	Wet	Dry	All	Wet	Dry	All	Wet	Dry	All	Wet	Dry	All	Wet	Dry	All	Wet	Dry	All
	Relative modulus of rupture	97.4	99.0	98.2	100.0	100.0	100.0	105.1	102.1	103.7	111.3	105.8	108.1	117.9	108.6	112.7	123.7	109.6	116.3	126.3	110.3	118.9
	Speed-strength modulus, T′	(0.014)	(0.005)	0.012	0.033	0.014	0.026	0.049	0.026	0.037	0.053	0.033	0.038	0.049	0.014	0.035	0.038	0.006	0.025	(0.023)	(0.004)	(0.014)

NOTE.—The usual speeds of testing at this laboratory are at rates of fiber strain of 15 and 10 ten-thousandths in. per min. per in. for compression and bending respectively.

much greater change in strength than the dry. Table III gives the ratio of the wet strength to that of the kiln dry at the several speeds. On the lower line of this table is given the inverse ratio of the modulus T for the wet and dry specimens at the same speeds. A remarkable correspondence is seen between these two ratios, moisture-strength and speed-strength. This may be simply a coincidence

TABLE III.—RATIOS OF ULTIMATE STRENGTH IN COMPRESSION AND BENDING, AT SEVERAL SPEEDS, BETWEEN THE SOAKED AND KILN-DRY MATERIAL; ALSO THE INVERSE RATIOS OF THE SPEED STRENGTH MODULUS T AT THE SAME SPEEDS.

	Rate of Fiber Strain. Ten-thousandths in. per minute per in.	⅔	2	6	18	54	162	486
Compression Parallel to Grain.	Speed of cross-head. Ins. per minute	0.000383	0.00115	0.00345	0.0103	0.0310	0.0931	0.270
	Ratio crushing strength, Dry ÷ Wet	....	2.80	2.74	2.67	2.60	2.50	2.41
	Ratio speed modulus, T wet ÷ T dry	....	(2.84)	2.75	2.24	1.96	2.60	(3.46)
Bending.	Speed of cross-head. Ins. per minute	0.0072	0.0216	0.0648	0.104	0.583	1.75	5.25
	Ratio modulus rupture, Dry ÷ Wet	2.00	1.97	1.91	1.87	1.81	1.74	1.70
	Ratio speed modulus, T wet ÷ T dry	2.80	2.36	1.88	1.61	3.50	6.34	5.75

The moisture condition of the kiln-dry material is given below, in per cent. of the dry weight:

	Compression.	Bending.
Longleaf pine	4.1	8.0
Red spruce	5.5	5.1
Chestnut	2.9	4.2

but it would appear to point to some relation as yet unknown. The sudden increase in the ratios of T for beams beyond a rate of fiber strain of 0.0018, is explained by the increased brittleness of the kiln-dry beams at the high speeds. The moisture-strength relation is seen from this table to be considerably modified by the speed of testing.

From the results of these experiments it is clear that the effect of speed must be taken into consideration in tests upon wood. Just how much variation in speed is permissible, without making

any correction, depends upon circumstance of the test. It must be noted, however, that while the range of values allowable in the results due to the great variability of the material in tests upon wood, is to a certain extent a compensating variable when many tests are made, the variation produced by a difference of speed, though it may be small is always a directive quantity.

There appears to be no simpler means of determining the value of the reducing factor for speed, than to use directly the emperical curves, or the speed strength modulus derived from them. Let us assume for an example, that the allowable error due to speed shall not exceed one per cent. of the strength; that the given speed corresponds to No. 7, or a rate of fiber strain of about 0.00135, and that the tests are to be made on green material in compression. Take the lowest curve in Fig. 3 and draw a tangent at point 7. Now draw a diagonal in the tangent diagram as explained previously, parallel to this tangent. Note that it intersects the 1 per cent. horizontal strength line at about 25 per cent. variation in speed. Hence the speed may be changed by 25 per cent.

Another example:—In testing as above at speed No. 7, what percentage increase in strength would occur due to an increase of 50 per cent. in speed? Draw the tangent and diagonal as before, and note that it intersects the 50 per cent. vertical speed line at about 1.8 per cent. variation in load, which is the answer. Or simply multiply 50 per cent, by the value of the speed strength modulus at the given speed.

An ordinary testing machine will be found to vary in speed greatly during a single test, and it is important to determine just how much variation in the strength this may produce. A change in speed of over 100 per cent. from start to finish of a heavy test is not unusual. This may be caused by stretching of belts, slippage, or changes in the motive power. The speeds upon which these curves have been based are the average speeds from the start to the maximum load. In some of the compression tests the speed up to the elastic limit was 40 per cent. less than the mean average at the maximum load. In the beam tests this variation was much less, not exceeding 5 per cent. of the mean.

No satisfactory comparisons with results obtained by impact tests have as yet been made. Two beam tests made upon this

material, however, one wet and one kiln-dry show increases in the elastic limit in bending of 172 per cent. and of 44 per cent. respectively, but no increase in the modulus of elasticity. These results are seen to fall in line at least qualitatively, with the others.

The total range of strength from the dead load to the impact tests is thus seen to be, very widely speaking, from one to four.

In selecting the proper speed at which to make the tests, other considerations will generally outweigh that of least variation in strength due to variations in speed liable to occur during the test, and it will generally be determined by the principles of economy of time combined with accuracy of measurements and operation. When there is a choice, however, preference should be given to the slower speeds.

The following speeds expressed as rates of fiber strain have been proposed as standards. Whenever a test differs from these rates of fiber strain sufficiently to cause a noteworthy change in strength the value should be reduced to the standard by means of accompanying curves.

For bending	$Z = .0015$	ins. per min. per in.
For compression	$Z = .0015$	" " "
For shearing	$Z = .010$	" " "

From Fig. 3 it will be seen that a change of 50 per cent. in speed will not in any case affect the load by more than a little over 2 per cent. Ordinarily, therefore, 50 per cent. may be regarded as a permissible change in speed without correction. In many cases even a much greater percentage change in speed would be permissible. In any case it may readily be found *by dividing the allowable variation in strength by the speed strength modulus.*

No test should be run at a rate of fiber strain to exceed 0.0150 ins. per min. per in. length of fiber for compression, and 0.0025 for beams, for beyond these points the brittleness of the material appears to effect the results. For convenience the formulas for ratio of fiber strain are repeated here and transposed to obtain speed of cross-head, n.

For compression or tension .... $n = l \times z.$

For bending, center loading .... $n = \dfrac{l^2}{6h} \times z.$

For bending, third point loading... $n = \dfrac{l^2}{4.7h} \times z.$

RECAPITULATION.

Summarizing briefly the main points of the discussion, we have the following:

1. The strength of wood varies significantly with the speed at which the stress is applied, increasing more rapidly as the speed increases. Therefore, all tests should be standardized for speed, and a means obtained for reducing values from one speed to another.

2. The *rate of fiber strain*, and not stress, should be used as the basis of reckoning, since rate of strain can be controlled, whereas the stress can not.

3. The Speed Strength Modulus, T, is the ratio of the relative change in strength to the corresponding relative change in speed. Hence, it is a coefficient which, if any given proportionate change in speed be multiplied by it, will give the proportionate change in strength at the speed in question. Its qualities are best shown by a logarithmic diagram where $x = \log V$, $y = \log L$, so that the value of the tangent to the curve at any point

$$\frac{dy}{dx}$$

is the value of the modulus T. Expressed algebraically,

$$T = \frac{dL}{dV} \times \frac{V}{dV} = \frac{d \log L}{d \log V} = \frac{dy}{dx}.$$

4. For no change in strength, the tangent and modulus would be zero, and for constant proportion between speed and strength, its value would be constant; i. e., the curve would be a straight line. Experiment shows that its value increases as the speed increases.

5. Wet or green wood shows much greater change in strength, than dry wood.

6. The modulus of elasticity in bending is practically constant. The fiber stress at elastic limit is nearly constant in compression parallel to the grain, but in bending it varies about the same as the ultimate crushing strength.

7. Standard rates of fiber strain have been proposed as follows:

Compression parallel to grain	$Z = .0015$	ins. per min. per in.
Bending	$Z = .0015$	" " "
Shearing	$Z = .010$	" " "

The amount of allowable variation from the standard should comport with the degree of accuracy of the rest of the operations. This may be found by dividing the allowable error by the speed strength modulus, or directly from the curves of Fig. 3. At least 50 per cent. change in speed may ordinarily be allowed without correction, for in no case would this give a variation in load of much over 2 per cent.

Whenever the speed differs from the standard by a significant amount, the values obtained from the test should be reduced to the standard speed.

No test, however, should be made with a rate of fiber stress in excess of 0.0025 ins. per min. per in. for beams and of 0.0150 for compression.

8. For tests on wood friction driven gears are to be avoided, and precautions taken to secure uniform speed throughout the test.

THE STRUCTURAL TIMBERS OF THE PACIFIC COAST.

By Rolf Thelen.

The growing scarcity of our timber supply, and the consequent increase in the price of lumber of all sorts and sizes is causing the consumers of forest products all over the country to search for cheaper woods which may be used as substitutes for those species now becoming dearer and harder to obtain. Naturally, the consumer looks toward the Pacific coast, with its immense forests of pines, firs, and other coniferous woods.

Although it is impossible to accurately estimate the amount of standing timber in the United States, most of the more recent estimates agree fairly closely. The following estimates of the stumpage of California, Oregon, Washington, Idaho, Montana, and British Columbia are from the *Pacific Lumber Trade Journal*, January, 1907.

	Thousand board feet.
Douglas fir	374,064,102
Western and yellow pine	175,586,520
Red cedar	78,961,383
Redwood	75,000,000
Western hemlock	60,848,259
Sugar pine	50,000,000
Spruce	25,419,215
Larch	5,078,601
Miscellaneous and hardwoods	5,700,000
Total	850,658,080

The State of Oregon is credited with over one-fourth of this total. The total estimated stumpage of southern yellow pine is but 275,000,000 thousand board feet, or practically 100,000,000 thousand board feet less than that of Douglas fir. The present annual cut of southern yellow pine is about one-twenty-third of its estimated total stumpage. This means that in about twenty-three years at the present rate, we may expect to find the southern yellow pine forests completely destroyed, even as the white pine

forests of Michigan have beeen destroyed. When that happens, the Pacific coast forests will be the only ones capable of furnishing structural timber of large sizes.

When the Forest Service first began the study of the physical and mechanical properties of the Pacific-coast timbers, there were practically no data whatever concerning them available. The Service has up to the present time made several thousand strength tests on these woods, and has established once and for all the average strength values for at least several of the species. Work on some of the other species is now in progress, and that on still others is about to be taken up.

METHODS OF TESTING.

Before entering upon a discussion of the strength values of the various species which have been tested, it may be well to briefly outline the general methods of test.

In those series of tests which are made with the object of obtaining the average strength values of any given species, full-sized bridge stringers are tested in bending. The first tests were made over a 16-ft. span with the beams loaded at the center point of the span. Now, however, all large beam tests are made over a 15-ft. span, with the load applied at the third points of the span by means of an auxiliary beam apparatus. After the main beam has been tested to failure, its uninjured portions are cut up into minor test pieces. These usually consist of several end compression specimens, several side compression specimens, several specimens for small flexure tests, and several small shear blocks.

The end compression specimens are 6 ins. square and 24 ins. long. The side compression specimens are 8 x 16 ins. in cross section, and are 24 ins. long. They are placed in the testing machine with the 16-in. edges vertical, and the load is applied through a 6-in. plate which rests across the top of the specimen. The area under stress is then 8 x 6 ins.

The small bending specimens are 2 x 2 ins. in cross section and 30 ins. long. They are clear and straight-grained, with the grain cut parallel to one face. These specimens are tested over a 28-in. span under center loading. After the test, the uninjured portions of these small beams are cut up into small end and side compression specimens. The small end compression specimens

are 2 x 2 ins. in cross section and 8 ins. long. The small side compression specimens are 2 x 2 ins. in cross section and 6 ins. long. The bearing plate used with these specimens is 2 ins. wide, making the area under stress 4 sq. ins.

The small shear blocks are clear and straight-grained, and are cut in such a manner as to fail in pure radial and tangential shear when loaded in the shear tool. This shear tool was especially designed for the Forest Service work, and is capable of testing blocks in single and double shear. The Forest Service tests are, however, all made in single shear.

The methods of test which the Service employs are fully described in its Circular 38, entitled "Instructions to Engineers in Timber Tests." They are but briefly outlined in the following paragraphs.

In all of the tests just mentioned, with the exception of the shear tests, deformation readings as well as load readings are taken. These load and deformation readings are taken at intervals frequent enough to give at least ten or twelve readings for each test. The readings are plotted on cross-section paper as the test proceeds. After the test has been completed, the load deformation curve is drawn in, and the elastic limit load is determined from it.

In the large bending tests, the deformation is obtained in the following manner: A nail is driven into the neutral axis of the beam directly above each support, and a thread or fine wire is stretched between these two nails in such a manner that it will remain taut during the test. A scale, graduated to hundredths of inches, is fastened to the neutral axis of the beam midway between the supports directly back of the thread. The deflection is read on the scale across the thread. There are two methods of doing this. One is to use a plain reading glass, and the other is to use a transit or similar telescope. With the former method, it is desirable to have a small strip of mirror fastened on to or parallel with the scale to facilitate accurate reading. With the latter method, this is not necessary. In view of the large deflections commonly obtained, it is sufficiently accurate to read deflections to the nearest hundredth inch.

Deformation measurements on large and small end compression specimens are taken between collars fastened to the specimens, and are read to the nearest ten-thousandth of an inch.

On the large specimens the collars are 20 ins. apart, and on the small ones they are 6 ins. apart.

Deformation measurements on large and small side compression specimens are taken directly on the movable head of the testing machine. They are measured to the nearest thousandth of an inch by means of a deflectometer of the usual design.

Since the moisture content of the specimen has a very marked effect on its strength, a moisture determination is made on every specimen tested. It has been found that in large timbers the seasoning process causes not only the loss of moisture but also the checking of the timber. Since it is impossible to determine the exact effect which this checking has on the strength of the timber, it is impossible to reduce the strength of the tested timber to that at some other condition of seasoning, even though we know exactly the effect of the moisture on the strength. Since it is necessary for the sake of comparison to eliminate entirely the effect of moisture, all large beams, with the exception of a few which are stored away to become thoroughly air seasoned, are now tested in the green condition. In this condition, variation in the moisture content has no effect upon the strength.

The weakening of large timbers by season checks nearly counterbalances the strengthening due to the reduction of the moisture content, so that there is very little gain, if any, in the strength of large beams due to seasoning. Working stresses should be based upon the fiber stresses developed by the green wood. and it is therefore not necessary to determine definitely the strength values of seasoned timber of large sizes.

RESULTS OF TESTS.

Douglas Fir.—The most important structural timber on the Pacific coast, and the one on which more tests have been made than any other, is Douglas fir.

Over three hundred 8 x 16-in. x 16-ft. Douglas fir bridge stringers have been tested up to the present time. A large proportion of these were tested green. These stringers were graded, in accordance with the export grading rules of the Pacific Coast Lumber Manufacturers' Association, into the three grades —select, merchantable, and second.

The green, select stringers developed an elastic limit fiber

stress of 4,370 lbs. per sq. in., a modulus of rupture of 6,720 lbs. per sq. in., and a modulus of elasticity of 1,660,000 lbs. per sq. in.

The merchantable stringers developed an elastic limit fiber stress of 4,030 lbs. per sq. in., a modulus of rupture of 6,140 lbs. per sq. in., and a modulus of elasticity of 1,510,000 lbs. per sq. in.

The seconds developed an elastic limit fiber stress of 3,590 lbs. per sq. in., a modulus of rupture of 5,200 lbs. per sq. in., and a modulus of elasticity of 1,340,000 lbs. per sq. in.

The average values for all these grades are: Elastic limit fiber stress, 4,050 lbs. per sq. in.; modulus of rupture, 6,140 lbs. per sq. in.; and modulus of elasticity, 1,526,000 lbs. per sq. in. The total number of stringers entering into this average is 135.

It is interesting to note that the average strength and stiffness values for the whole 135 stringers are almost identical with those for the merchantable grade. Out of these 135 stringers 26 failed by longitudinal shear or splitting along the neutral axis.

The lumbermen of the Pacific Northwest commonly divide Douglas fir into two kinds, known respectively as red fir and yellow fir. The distinction is purely one of color, a single tree often furnishing both red and yellow fir. In general, the red fir is the coarser-grained wood near the heart of the tree, and the yellow fir is the finer-grained wood near the bark of the tree.

To determine definitely whether the color has any effect on the strength of the wood, an analysis was made on 256 Douglas fir stringers. These were divided into two groups on the color basis.

There were 94 yellow fir stringers and 162 red fir stringers. Of the former 47.8 per cent. were selects, 40.4 per cent. were merchantables, and 11.8 per cent. were seconds. Of the latter 29.8 per cent. were selects, 43.8 per cent. were merchantables, and 26.6 per cent. were seconds. Grade for grade, the tests show that there is practically no difference between the strengths and stiffnesses of red and yellow Douglas fir stringers. The average elastic limit fiber stress of the select yellow stringers was 101 per cent. of that developed by the red selects. The same quantity in the yellow merchantables was 101 per cent. of that for the red merchantables, and for the yellow seconds was 99 per cent. of that for the red seconds. The difference is so very slight that it may safely be neglected.

There are a great many factors which influence the strength and other properties of wood. Of these, the principal ones are specific gravity, moisture, and defects. Especially does the last named assume importance in structural sizes.

An analysis was made upon a large number of Douglas fir bridge stringers to determine the exact effect which the size and position of knots has on the strength of large beams.

Each beam was divided into three volumes. Volume 1 included the central two-thirds of the beam, lengthwise, from the bottom face upward one quarter of the height of the beam. Volume 2 was the corresponding part of the beam measured from the upper face. Volume 3 was, then, the remaining portion of the beam.

In general, the fibers in Volume 1 are subjected to heavy tension, those in Volume 2 to heavy compression, and those in Volume 3 are subjected mainly to shear and to slight tension and compression.

After the beams had been divided into these three volumes, they were grouped into three groups as follows: All beams containing defects in Volume 1 were put into Group 1. Those having defects in Volume 2 but not in Volume 1 were placed in Group 2. The remaining sticks were placed in Group 3.

To state the matter briefly, Group 1 contained all sticks having defects in the volume under heavy tension, Group 2 those sticks containing defects in the volume under heavy compression, and Group 3 those sticks containing no defects in the volumes under heavy tension and compression. The average strength values of each group were then determined. Assuming that the average elastic limit fiber stress of Group 3 was 100 per cent., that of Group 2 was 92.5 per cent., and of Group 1 was 80.1 per cent. However, all of this difference was not caused by knots alone. The stringers of Groups 1 and 2 were not of as good general quality as those of Group 3. The wood was lighter and hence weaker. The tests on the small, clear sticks cut from the large beams after test show that at the elastic limit the clear wood of Group 2 was 97.6 per cent. as strong as that of Group 3, while that of Group 1 was only 90.5 per cent. as strong as that of Group 3. It is quite logical that this should be the case, since knots are more usually found in the lighter, more rapidly growing wood.

Correcting these values, then, for influences other than those of mere defects, we find that knots in that part of large Douglas fir beams which is subjected to heavy compression reduce the strength of the beams about five per cent., while those in the corresponding tension side reduce the strength about ten per cent.

These statements are of course very general in their nature, and can hardly be applied rigidly in any given specific case.

It is a well-known fact that the strength of large beams is less in proportion to their size than that of small, clear sticks. In the green state this is due to a large extent to the presence of defects. Since it is impossible to properly season large beams, no logical comparison between large and small sticks can be made except in the green state.

Comparing the values of 135 green Douglas fir stringers with those of the minor bending specimens cut from them, we find that the fiber stress of the large beams at the elastic limit is 71 per cent. of that of the small beams, while their modulus of elasticity is 87 per cent. of that of the small beams.

Compression tests parallel to the grain are made as previously stated upon two sizes of specimens, 6 x 6 x 24 ins. and 2 x 2 x 8 ins. The larger specimens may contain defects. The smaller ones are always clear and straight-grained.

The average values for 428 large, green specimens of Douglas fir tested in end compression are: Compressive strength at the elastic limit, 2,840 lbs. per sq. in.; and crushing strength at maximum load, 3,590 lbs. per sq. in. The average compressive strength at the elastic limit for 503 small, green specimens was 3,944 lbs. per sq. in., or 39 per cent. greater than the corresponding stress for the larger specimens. Expressed the other way, the larger specimens were 72 per cent. as strong as the small ones. For bending, this same ratio was 71 per cent.

Three hundred seventy-four tests in side compression or plate bearing on partially air-dry Douglas fir showed an average fiber stress at the elastic limit of 651 lbs. per sq. in. of bearing area.

There is practically no difference between the radial and tangential shearing strengths of Douglas fir. Seven hundred and fifty-eight tests on partially air dry material averaged 770 lbs. per sq. in. maximum shearing stress.

Western Hemlock.—The Forest Service has made a large

number of tests on western hemlock. The wood of this species is valuable for a great many purposes on account of its strength, resistance to splitting, adaptation to staining and treating processes, and other admirable qualities. However, the mere fact that it is named "hemlock" has been, up to the present at least, sufficient to prevent it from gaining a permanent place of its own in the market. The Forest Service tests show that western hemlock is stronger in bending than loblolly pine, shortleaf pine, Norway pine, and tamarack. Thirty 8 x 16-in. x 16-ft. green western hemlock beams of all grades showed an average elastic limit fiber stress of 3,738 lbs. per sq. in., and an average modulus of elasticity of 1,475,000 lbs. per sq. in. This fiber stress is 92½ per cent. of that developed by the green Douglas fir beams mentioned before. It is interesting to note by way of comparison that 40 per cent. of these stringers were seconds, and only 30 per cent. each were merchantables and selects, while 40 per cent. of the Douglas fir stringers were *selects*, and only 23.7 per cent. were seconds! In other words, the average grade of the hemlock was poorer than that of the fir. The comparison, therefore, is not quite fair to the hemlock.

One hundred thirty 6 x 6-in. specimens of partially air-dry western hemlock tested in compression parallel to the grain developed an average elastic limit fiber stress of 2,840 lbs. per sq. in., and a crushing strength at the maximum load of 3,705 lbs. per sq. in. This elastic limit fiber stress is 90½ per cent. of that developed by 422 partially air dry Douglas fir posts. The latter averaged 4.9 per cent. dryer than the hemlock.

In side compression, 115 partially air dry western hemlock specimens developed an average elastic limit fiber stress of 477 lbs. per sq. in.

Western Larch.—Western larch is one of the woods most recently tested by the Forest Service. Although but few tests have so far been completed, these seem to indicate that western larch is a structural timber of the highest quality; that it ranks nearly in the same class with Douglas fir and longleaf pine.

Nine 8 x 12-in. x 16-ft. green western larch beams of clear and merchantable grades developed an average modulus of rupture of 6,140 lbs. per sq. in. This is exactly the modulus of rupture shown by all the 8 x 16-in. Douglas fir stringers.

In compression parallel to the grain, 36 large green western larch specimens developed an average crushing strength of 3,641 lbs. per sq. in. This is 101.5 per cent. of that developed by the large Douglas fir compression specimens.

Thirty-eight side compression specimens of green western larch showed an average elastic limit fiber stress of 484 lbs. per sq. in.

Redwood.—Tests on redwood have just been begun by the Forest Service. No results are as yet available. However, a series of tests conducted at the University of California laboratory in 1901 indicate that redwood is a timber of a high order of structural merit, ranking with Douglas fir and longleaf pine in strength.

Miscellaneous Western Woods.—Although the Service has not yet made any tests on large sizes of lodgepole pine, white fir, Engelmann spruce, alpine fir, and western yellow pine, it has made several hundred tests on small, green end compression specimens. The results of these tests are of interest in that they show a comparison of the strengths of these species in small sizes. There are too few tests so far to give definite values. The following table shows the crushing strength of these species as determined by the small tests just mentioned, as well as that of the small, green Douglas fir end compression pieces:

Species.	Number of tests.	Crushing strength lbs. per sq. in.
Douglas fir	503	3,944
Lodgepole pine	87	2,704
White fir	97	2,595
Engelmann spruce	117	2,335
Alpine fir	44	2,193
Western yellow pine	79	2,116

Eucalyptus.—The last species to which the writer wishes to call your attention are the eucalypts of California. Although they are hardwoods, and do not grow in such large sizes as Douglas fir and western hemlock, for instance, there is no valid reason why they should not be classed with the structural timbers, for at least several of them readily grow to a diameter of 30 ins., and under forest conditions form truly long-shafted trunks free from branches. Blue gum is the fastest growing of the eucalypts, it

not uncommonly adding an inch per year to its diameter. Since the eucalypts are hardwoods, it may be well to compare them with hickory. A comparison of a small number of eucalyptus tests with tests on hickory shows that 30-year old blue gum is stronger than XXX hickory, and that 15-year old sugar gum is nearly as strong as black hickory, and 91 per cent. as strong as second-growth hickory.

Most of the data in this paper have been published in recent Forest Service publications. Of these, the principal one is Circular 115, entitled "Second Progress Report on the Strength of Structural Timber." This publication is available for distribution and may be obtained by request. A list of the available Forest Service publications may also be obtained by addressing the Forester, Washington, D. C.

THE ACCEPTANCE OF STONE FOR USE ON ROADS BASED ON STANDARD TESTS.

By R. S. Greenman.

That the people who reside in the rural districts are beginning to fully realize the value of good roads there is no doubt; that those people who reside in cities and who have the means of getting into the country districts desire good roads there is less doubt; and that the people in general are awakening to the fact that rural roads have been too long neglected is a self-evident fact. Because of the general desire for better roads we find some sections of the country either spending, or planning to spend, considerable money in the construction of new roads. Local conditions of course, control the character of the road to be built but as a general rule the desire is to construct macadam roads.

If macadam roads are such as appeal generally to the public, and if the money of the public is to be spent in building this kind of road, all possible precautions should be taken to secure the best results. In any single case a matter of poor judgment in selection of method or material does not appear to amount to much, but when a state undertakes to spend from five to fifty million dollars on new roads there is a possibility of either saving or losing much. As an approximate estimate it may be figured that twenty-five per cent. of the cost of a new road is the cost of the stone for the macadam. The selection of stone then is one of the most important preliminary steps in constructing a new road.

In making a selection of stone there are necessarily several conditions which must be considered. The greatest of these is the amount and kind of traffic, then the availability of good road metal, and not least is the item of cost. This paper is not to be an economic discussion of road building, but reference must be made to such conditions as enter into the problems presented in considering the subject of "The Acceptance of Stone for Use on Roads Based on Standard Tests." If, therefore, we are to thoroughly understand our subject we must consider some of the problems of road construction.

Concrete examples always help to illustrate, so here reference to problems will mean some of those that confront the Department of the State Engineer and Surveyor in the State of New York in an endeavor to establish a complete system of tests and satisfactory standards or limitations in the acceptance of stone. The great State of New York has become well established in the construction of fifty million dollars worth of new roads. Experience has already taught that greater care must be taken in the selection of the road metal. The present State Engineer has placed in the laboratory of his department an outfit for the complete testing of stone. This laboratory now contains duplicates of the machinery and apparatus in the laboratory of the Office of Public Roads, Department of Agriculture, at Washington; and the tests that have been standardized at this latter laboratory are being followed minutely at the State Laboratory in Albany.

The real question before us is not how to make tests and secure comparisons, but, having secured the results, how to draw limitations in the selection of stone that will be fair to both the state and the producer. In other products we have standard requirements but in this field we have yet to develop just limitations. Based on standard tests it would be practically impossible to draw up a general specification that would permit the use of a good limestone, and that would at the same time shut out a poor trap-rock or a poor syenite.

It may be necessary that in each case local conditions shall first be considered and then a specification made to fit these conditions. With a large amount of roads about to be constructed this probably would not be entirely satisfactory, so if specifications can be drawn covering general conditions and applying to the general classes of road construction, better results must be the outcome of such specifications. For the purpose of discussion it may be well to divide roads into three general classes: Those of the first class, or grade, will be those that are "interurban" or "trunk lines" which are subject to heavy traffic and wear and which shall be made the best possible with cost a subordinate consideration; the roads of the second grade will be such roads as are subject to the traffic of an average popular country road, and which shall be constructed with due consideration to both wearing qualities and to cost; and the third grade roads will be those which

have the ordinary country use and of which the cost of construction and maintenance shall be kept as low as possible.

To secure a suitable stone for use on a first grade road, as suggested, it would be necessary to make a requirement so severe that only such road metals as trap rock or syenite would pass the requirement. Only such stone as would at least reach the "high" classification used in reporting the tests of the Office of Public Roads could really be considered for use in this class since for a road of this grade for which the cost is a secondary condition, we would expect good things of the stone. If a positive requirement in the results of the tests is to be made we would scarcely wish to accept a stone for use that would not show at least a coefficient of wear of 15, a hardness of 16, a toughness of 13 and a cementing value of 25. We should expect that a good stone would show far better results in some of the tests than the figures given, but it should not fall below in any one of the tests. The question of just what cementing value to expect from a hard and high grade road stone is worthy of special consideration and will be treated later.

For a second grade road the requirements should take a much more liberal character. In constructing a road of this grade cost would be a much more important item and the natural desire would be to use the best local stone; but if the quality of the road is to be maintained it will probably be necessary in a majority of the cases to "import" a desirable stone. It is for this grade that there should be a wide range in the limitations, and yet the minimum requirements should be severe enough to shut out a decidedly inferior stone. We would not want to accept a stone that would not give results of at least 10 for coefficient of wear, 14 for hardness, 10 for toughness, and 20 for cementing value.

A road of the suggested third grade could be built of any local stone that might be obtainable as long as it had any of the needed qualities of a road metal. The low limitations could be just severe enough to shut out any stone that had weathered badly or had not shown any merits for use as road stone.

The suggestions that have been made are made merely for purposes of bringing out discussions, but they have been suggested as the result of the experience of the Department of the State Engineer of New York in his efforts to select the best stone for use

on the new roads. The tests of the stone have given results that aid greatly in selecting and accepting a stone but the value of the tests is to be more greatly developed as the use is extended. There can be no question but that, if it is positively settled before a contract is made that a stone is acceptable for use on the new road and the contractor as well as the state so understands the case, there will be better satisfaction and better roads. So it would seem to be an ideal condition if the state would make a thorough examination of the stone supply and could tell bidders that certain stones have stood tests that make them acceptable and any one of several stones may be used. To protect its interests after the work has been started, the state might require that tests be made of the stone being used from time to time and that a fair sample shall not fall below a certain per cent. in the tests as compared with the original.

It has been understood in this discussion that the tests shall be those which have been standardized by the Office of Public Roads, United States Department of Agriculture. An argumentative treatment of the tests will not be made at this time. It should be said, however, that these tests can be of very great value to builders of good roads. It is often the case, however, that a practical road builder will prefer to rely upon his experience regarding a stone rather than upon the results of laboratory tests; nevertheless, comparisons between these results and the results in actual use show that the tests forecast the ultimate results in the road.

Of these tests perhaps the one for testing cementing value gives the cause for greatest criticism. This test often gives results that the practical road builder does not experience in the use of the stone. As a rule the harder stones do not give good first results when used as a binder. In tests, however, these very same stones may give a high cementing value. The difference is undoubtedly due to the fact that in preparing the "dough" for the cementing value test the stone is ground much finer than when drawn from the crusher as "screenings." Although this test may not give results that are as close to actual results in process of road construction, still the test helps with the others in forming an opinion and a decision regarding the stone.

That the laboratory tests of road stone can be made to give

the same protection in the use of the stone that is secured in tests of other materials there can be no doubt; that the placing of limitations, beyond which the stone must pass before it may be used, will follow the general adoption of the standard tests, and that those states, or other large builders of roads, that avail themselves of the results of tests, will secure the better roads, is but the experience of the past in connection with other materials, and is bound to be the experience of the future regarding road metals, perhaps not in just the way that has been suggested in this discussion, but in a way that will have a real practical value to the builders of roads.

DISCUSSION.

MR. L. W. PAGE.—Probably the most serious problem that has ever been considered by the highway engineers has been brought about by automobiles. The macadam road has been developed to withstand iron-tired horse vehicles, and under ideal conditions the iron tire wears off an amount of the rock composing the bed of the road sufficient to furnish a binder for the coarser stone to replace that carried off by natural agents, such as wind and water. Now the automobile has upset those conditions completely. The soft pneumatic tire wears off practically no dust from the coarser fragments composing the road, and the driving wheels of these powerful machines exert an enormous tractive or shearing force on the surface of the road, and throw the dust that should act as a binder into the air, where it is carried off by the wind upon adjacent property. In many cases the value of real estate has decreased in consequence. It loosens the bed of the road so that water makes its way rapidly to the foundation and the road ravels very quickly, as the roadbuilder calls it, and disintegrates. In many places in this country the cost of maintenance has increased over one hundred per cent. It is such an important problem that the French Government this year has called an international congress to consider this one problem. Over $600,000,000 has been spent in France on 24,000 miles of macadam road, and they are being ripped to pieces so rapidly by automobiles that if they do not take some drastic action the whole principal that has been expended will be lost. **Mr. Page.**

MR. A. S. CUSHMAN.—I should like to say a word about the cementing value of rock dust. We have been observing the result of the cementing value test for a number of years. We feel that this test is one that is open to criticism, and yet its study has probably taught us more about the different qualities which make stone appropriate for road building than any other test. It has been found that many rocks when ground up with water, and molded into briquettes, disintegrate rapidly when the dried briquettes are subsequently immersed in water. This property is **Mr. Cushman.**

Mr. Cushman. referred to as "slaking." It is a fact that this property of slaking is not confined to any one kind of rock. We find a great variation in the property of slaking in the same kind of rock. Some limestone will yield briquettes that do not slake at all, even after indefinitely prolonged immersion. Others will fall away in a very fine powder in a few moments. Then there are others that will range along between those that take a few minutes and those that take a half an hour to slake. The same thing is true of other types of road building stone. We cannot help feeling that that must be an important property of road material. If a rock dust which forms the binder surface on a macadam road, when it is wet with rain, proceeds to disintegrate on the surface, the conditions are present for a muddy road in wet weather; whereas, a non-slaking rock dust should make a rock binder that does not lead to muddy conditions.

It is found too that the slaking is modified by the degree of drying. That is to say, if the briquettes are allowed to dry in air they will gradually lose the power of slaking, so that in about twenty or thirty days, in ordinarily fairly dry air, some rock powder will absolutely lose the power of slaking. Others, however, will not. But all briquettes that are dried at 100° for four hours have their bond formed. In our tests the briquettes are dried at 100° for four hours and then allowed to cool, before they are put into water.

Mr. Greenman. MR. R. S. GREENMAN.—In regard to the cementing value of stone dust, we have found in the State of New York that as a rule the road builders, both the contractors and engineers in charge of the work, are under the impression that the trap-rock does not bind well. I suppose that often the reason is that they do not want to attempt to give the time and labor to get the full result of the binding qualities of that stone. Tests show that trap rock has cementing value on the road as well as in the laboratory. In order to prove that that is so, we have started to make a series of tests. It consists of taking the stone dust as it comes from the crusher and trying the cementing value of that without grinding it in our laboratory mills. These tests confirmed the results that we got from the regular laboratory tests, although not giving as high results. I think we are going to be able to convince road builders that there is a greater cementing value in harder stone than many are now willing to admit.

Mr. Page.

MR. PAGE.—I fail to see how that test can prove of any material value in determining the cementing value of rock dust, for the reason that it is not the comparatively coarse grindings that come from the crusher that bind. The road has to be rolled with a steam roller and wetted frequently. After it has been driven over for some time the very fine dust that binds these fragments together is formed.

FUEL INVESTIGATIONS, GEOLOGICAL SURVEY. PROGRESS DURING THE YEAR ENDING JUNE 30, 1908.

By J. A. Holmes.

The investigation of fuels by the United States Government had its beginning in an act of Congress providing for the analyzing and testing of the coals and lignites of the United States at the Louisiana Purchase Exposition in 1904, under the supervision of the Director of the United States Geological Survey. The work was continued at St. Louis until early in 1907, when the settlement of the affairs of the Louisiana Purchase Exposition necessitated the removal of the buildings from Forest Park, and consequently the removal of the fuel-testing plant. At this time the Jamestown Exposition at Norfork was approaching completion, and the exposition authorities offered, free of charge, the use of the necessary buildings, grounds and other facilities for continuing the work. The railroad lines offered to transport from St. Louis to Norfolk, free of charge, all necessary machinery and other equipment.

Meanwhile, a coöperative arrangement having been entered upon between the Departments of the Interior and Navy relative to the testing of coals suitable for naval purposes, it was decided to transfer to and operate at Norfolk all of the fuel-testing equipment except that for coking and washing tests, which was removed to and erected at Denver, Colo., for use in testing the coals of the Rocky Mountain states—especially those in lands belonging to the government.

The expediency of this removal of the plant was enhanced by the need of a brief period of inactivity in testing so as to allow time for computing the results and preparing the necessary reports.

In connection with the work at Norfolk it seemed highly desirable to make steaming and briquetting tests of the coals which reach the Atlantic seaboard and are used by the navy and the merchant marine. It was also considered important that these investigations at Norfolk should include the possibilities of abating the smoke nuisance on naval vessels and in various public buildings

burning bituminous coal. At the same time it was felt that this was an opportune time for investigating the possibilities of producing high-grade metallurgical coke from the coals of the Rocky Mountain regions, especially those in the public lands, and it was for this purpose that the coke and washery sections were erected at Denver.

Meantime, for convenience in analyzing the fuels reaching the various testing plants, chemical laboratories were operated temporarily in Columbus, Pittsburg, Norfolk, Denver, Berkeley, and in the office of the Geological Survey in Washington, the latter for the special analyses of fuels purchased by the government.

PLAN AND SCOPE OF THE FUEL INVESTIGATIONS.

The scope of the fuel investigations has been planned to conform to the law which provides for the analyzing and testing of coals, lignites and other mineral fuel substances belonging to the United States, or for the use of the Government of the United States, and examinations of fuels representative of extensive deposits for the purpose of increasing the general efficiency or the available supply of the fuel resources in the United States.

The following are the general plans and purposes of the investigations undertaken and contemplated by the technologic branch, after conference with the National Advisory Board and with its advice and approval.

A. The ascertainment of the best mode of utilizing any given fuel deposit belonging to the government or to be used by the government, by investigations of its combustion under steam boilers, its conversion into producer gas, or into coke or briquettes.

B. The prevention of wastes in mining, shipping and utilization of fuels.

C. The inspection and analyses of coal purchased under government specifications.

The work of determining the most efficient and most economical method of utilizing the fuel resources of the United States has developed along the following lines:

1. The collection of representative samples for chemical analysis and calorimeter tests from the mines selected as typical of extensive deposits of coal; the collection from the same mines of large samples of one to three carloads of coal for various tests.

2. The testing of each coal received to determine the most efficient methods of using the same in different types of furnaces.

3. The testing of another portion of the coal in the gas producer to determine its availability for use in such producers for power plant purposes.

4. The testing of other portions of the coal in briquette machines, to determine the feasibility of briquetting the slack coal.

5. The testing of high ash or sulphur coals in the washery plant in order to ascertain whether this treatment improves the coal sufficiently to repay the cost of washing.

6. The testing of other portions of the coal, before and after washing, in bee-hive coke ovens to determine the best methods of producing large output and good quality of coke.

7. The testing and analysis of lignite.

8. The testing and analysis of peat.

9. The testing and analysis of petroleum products.

10. Special research work in connection with every branch of the testing.

The investigations into the waste in mining and the testing of the waste products in the gas producer and under the steam boiler as briquettes have for their purpose to demontrate how these materials, now wasted, may be profitably used for power purposes.

The inspection and sampling of coals delivered to the government under purchase contracts is made with the intent of determining whether or not the specifications have been complied with. In every case a chemical analysis and a determination of heating value are made.

The research work embraces special investigations of coal, such as the changes in the transformation of peat to lignite, the chemical and physical processes in combustion, destructive distillations, by-products of coking processes, spontaneous combustion, storage of coal, and the weathering of coal.

SOME RESULTS OF THE FUEL INVESTIGATIONS.

Important results to which these investigations have already led may be briefly summarized as follows:

The purchase of coal under government specifications has resulted in the delivery of a better grade of fuel without a corresponding increase in cost.

Suggested changes in equipment and methods indicate the practicability of the use of cheaper fuels by the government.

The chemical analyses and calorific determinations of coals from nearly a thousand different localities form an invaluable source of accurate information as to the different coal-fields of the United States.

The tests of different coals under steam boilers have shown the possibility of increasing the over-all efficiency of hand-fired plants 10 to 15 per cent. above that ordinarily attained.

The results of the gas-producer tests show that many fuels, such as slack and bone coal, practically valueless for steaming purposes, may be economically converted into producer gas and thus generate sufficient power to render them of high commercial value. On an average there was developed from each coal tested in the gas producer plant two and one half-times the power developed when the same coal was tested in the steam boiler plant. North Dakota lignite developed as much power when converted into producer gas as did the best West Virginia bituminous coal when burned under the steam boiler.

The tests made with reference to the manufacture and combustion of briquetted coal have demonstrated the commercial value of many low-grade bituminous coals and lignites, and that also high-grade bituminous coals may be burned in locomotives in the form of briquettes with increased efficiency and with less smoke than the same coal unbriquetted.

The smoke abatement investigations show that any kind of coal may be burned practically smokelessly under a wide range of conditions.

The investigations into the waste of coal in mining have shown the enormous extent of this waste, at least half of which might be avoided through more efficient mining methods.

The investigations into mine explosives and explosions have indicated how future work should be conducted to avoid unnecessary waste of material and loss of human life.

The washing tests have demonstrated the fact that many coals, too high in ash and sulphur for use under the steam boiler or for coking, may be rendered of commercial value.

The coking tests have demonstrated that a great many coals, which were formerly regarded as non-coking, may be made into

good coke in the bee-hive oven when properly prepared and manipulated.

The investigations into the relative efficiency of gasoline and denatured alcohol have shown that with proper manipulation denatured alcohol has the same power-producing value, gallon for gallon, as gasoline, although the heating value of a gallon of alcohol is only a little more than six-tenths that of a gallon of gasoline.

Several hundred tons of peat have been tested to determine methods of drying, compressing into briquettes, and utilization for power purposes. A reconnaissance survey of the peat deposits of the Atlantic Coast has been made, and samples have been obtained and analyzed to determine their nature and their fuel value.

THE PUBLICATION OF RESULTS.

The results of tests so far made in the course of these investigations have been published in the following professional papers and bulletins:

B 261. Preliminary Report of the Operations of the Coal-Testing Plant of the United States Geological Survey at the Louisiana Purchase Exposition, St. Louis, Mo., 1904.

PP 48. Report on the Operations of the Coal-Testing Plant of the United States Geological Survey at the Louisiana Purchase Exposition, St. Louis, Mo., 1904. (Three parts.)

B 290. Preliminary Report on the Operations of the Fuel-Testing Plant of the United States Geological Survey at St. Louis, Mo., 1905.

B 323. Experimental Work Conducted in the Chemical Laboratory of the United States Fuel-Testing Plant at St. Louis, Mo., January 1, 1905, to July 31, 1906.

B 325. A Study of Four Hundred Steaming Tests Made at the Fuel-Testing Plant, St. Louis, Mo., in 1904, 1905 and 1906.

B 332. Report of the United States Fuel-Testing Plant at St. Louis, Mo., January 1, 1906, to July 1, 1907.

B 333. Coal-Mine Accidents: Their Causes and Prevention. A Preliminary Statistical Report.

B 334. The Burning of Coal without Smoke in Boiler Plants. A Preliminary Report.

B 336. Washing and Coking Tests of Coal and Cupola Tests of Coke at the United States Fuel-Testing Plant, St. Louis, Mo., January 1, 1905, to June 30, 1907.

B 339. The Purchase of Coal under Government and Commercial Specifications on the Basis of its Heating Value. 1908.
B 343. Binders for Coal Briquettes. Investigations Made at the United States Fuel-Testing Plant, St. Louis, Mo., 1905.
B 316. Extract from General Papers on the Producer-Gas Power Plant; the Coal-Briquetting Industry, and Coal-Mine Sampling. 1907.

In addition to these publications, the following bulletins on the following subjects are in press:

1. Results of all the chemical analyses, and the field-sampling on which they are based.
2. Causes of waste of coal in mining.
3. Results of the combustion of briquettes on locomotives.
4. Results of all gas-producer tests.
5. Results of all steaming tests since Professional Paper 48.
6. Results of comparative gasoline and alcohol tests.
7. Mine explosions.
8. A final report of the smoke abatement investigations.
9. Comparative tests of coal and pitch briquettes in house-heating boilers.

COMMERCIAL RESULTS IN THE PURCHASE OF COAL ON SPECIFICATIONS.

By J. E. Woodwell.

The paper presented by the writer at the last meeting of the Society reviewed a number of specifications in general use for the purchase of coal and presented particularly the specification which has been approved by the National Advisory Board on Fuels and Structural Materials, appointed by the President, and adopted by the United States Treasury and other departments of the government.

The object of this paper is to present the results of recent experience in the purchase of coal for the Treasury and other departments of the government through the application of the government specification.

A schedule of the contracts for coal for the present fiscal year is shown in Table I which states the city in which delivery is made and gives a description of the coal and the contract data of tonnage, price, thermal value of the coal as received, and percentage of ash in the dry coal, all as stipulated by the bidder in the accepted proposal. For the sake of offering a ready means of comparison, the theoretical thermal value has been computed in terms of the cost of one million B. T. U.

All of the coal listed in Table I is subject to delivery in small lots through the contract period of one fiscal year to the public buildings of the city named. In making comparisons, therefore, with the cost of coal f. o. b. cars or in wholesale lots, allowance must be made for cartage and trimming bunkers, which is included in the prices named.

Payment for the coal delivered is made monthly in each case based upon the proposal price per ton, corrected, first, in direct proportion to the calorific value of the coal delivered compared with the contract standard, and, second, for variation from the contract standard of ash in dry coal in accordance with the specifications referred to.

The monthly deliveries of coal are systematically sampled by

taking small quantities at regular intervals as the wagons are being dumped into the bins. The samples are procured in such manner as to include lump and fine coal in about the same proportion in which they occur in the lot delivered and average about 15 lbs. for 15-ton lots, so that a monthly delivery of 500 tons would be represented by a sample of at least 500 lbs. The monthly sample accumulated and stored in a tight metal receptacle is broken down, thoroughly mixed and quartered successively until a final sample of one quart size is secured and forwarded to the laboratory for analysis and test, under the supervision of the Technologic Branch of the Geological Survey.

The results of the tests for the current fiscal year are shown under the head of contract fulfilment in Table I. It will be noted that the contract standard of heating value was fulfilled in only five of the thirty-two contracts, though the departure was less than 5 per cent. in all but eight cases. Of the eight exceptions, four related to low-grade anthracite coals and three to Illinois and Kentucky crop coal and one to a Pennsylvania coal. It is probable that due allowance was not made in a number of cases for the difference between guarantees based upon the heating value of the coal as delivered or received and that based upon the *dry* coal. The moisture content in coal is an important factor in contracts which recognize the heating value and other elements as a basis of payment and under the plan established in the government specification no separate correction for moisture is made but the heating value of the coal is determined in the condition in which it is received, containing whatever moisture may be present at the time. The plan presupposes, however, the original establishment of a standard by the bidder which shall represent, as far as practicable, the actual delivery conditions so far as the effect of moisture is concerned.

It is obvious that under this system neither the contractor nor the purchaser will gain or lose by change in the moisture content of the coal between the time it is weighed at the mine and the time it is weighed on delivery. The price per ton will be correspondingly lower if the coal is wet, and higher if the coal is dry.

For purposes of comparison, the average percentage of moisture in the coal as delivered under the various contracts is listed in Table I. In the contracts for Pennsylvania, Maryland and Vir-

TABLE I.—COAL CONTRACTS. U. S. TREASURY DEPARTMENT. FISCAL YEAR 1908.

CITY	DESCRIPTION OF COAL	CONTRACT BASIS					CONTRACT FULFILMENT				
		TONS 2240 LBS.	PRICE PER TON	B.T.U. COAL AS RECEIVED	ASH IN DRY COAL PER CENT	COST OF 1000 000 B.T.U. CENTS	TONS DELIVERED	NO OF TESTS	AV. B.T.U. COAL AS RECEIVED	AV ASH IN DRY COAL PER CENT	AVERAGE MOISTURE PER CENT
BALTIMORE	BIT VINTONDALE PA	2250	$3.31	14200	8	10.406	1791	10	14130	7.84	2.18
"	" "	350	3.41	14200	8	10.720	292	5	14521	8.01	2.01
BOSTON	BIT POC N.R. OR G C	1500	5.40	14750	6.25	16.343	1318	10	14190	6.43	3.40
"	ANTH SCR PHIL & READING	3000	2.80	12650	12.00	9.881	2301	10	12697	10.91	4.97
BROOKLYN	No 2. BUCK PITTSTON PA	3800	3.25	12711	13.27	11.415	3191	11	11110	19.20	5.68
BUFFALO	BIT OAK RIDGE, HILLVILLE PA	1350	2.90	13.770	4.75	9.402	862	7	12543	13.40	3.82
CINCINNATI	BIT NEW RIVER, SEWELL SEAM	3000	3.00	14700	5.00	9.711	2658	10	14574	5.67	2.02
DETROIT	BIT KANAWHA OAKLAND W.VA	1400	3.30	14000	4.36	10.523	1189	9	13440	9.07	3.66
LOUISVILLE	BIT ST CHARLES & FOX RUN KY	1850	1.95	12200	11.50	7.136	1416	10	11458	13.82	7.97
MILWAUKEE	BIT YOUGH SCR (PA)	1600	3.40	13466	9.67	11.272	1192	7	12909	10.39	4.45
MINNEAPOLIS	BIT ZEIGLER No 1 (ILL)	500	4.70	12250	8.60	17.128	550	8	11927	9.90	8.56
NEW ORLEANS	BIT PRATT CITY ALA	1050	3.75	14000	8.50	11.958	981	10	13916	9.36	1.34
NEW YORK	BIT ACME, HAWK RUN PA	2500	3.46	14474	7.80	10.672	2067	9	14022	7.93	2.58
" "	ANTH SCR ONT & WESTERN	3500	2.48	12998	11.68	8.518	2053	10	12105	14.58	5.33
" "	BIT DELTA SPANGLER PA	10000	3.45	14315	6.80	10.759	9300	20	13982	8.39	2.64
" "	ANTH BUCKWHEAT No 2.	2240	3.10	12711	13.27	10.888	2232	5	11261	19.11	5.56
" "	ANTH PEA DEL & HUDSON.	500	4.15	11662	17.49	15.887	336	3	11629	18.44	4.34
OMAHA	BIT FLEMING MR KAN	1700	4.69	12200	13.00	17.162	1323	8	12709	11.91	4.21
PHILA	BIT ACME HAWK RUN PA	1500	3.58	14474	7.80	11.042	1520	7	13861	9.63	1.92
"	" " " "	4500	3.18	14474	7.80	9.808	2958	10	13980	9.37	3.06
"	ANTH PEA DERRINGER COAL	350	3.59	11651	16.77	13.756	316	7	11814	16.35	4.71
"	ANTH BUCKWHEAT	1500	3.35	11855	20.81	12.615	1002	4	11116	20.91	5.07
ST LOUIS	BIT STAUNTON LUMP ILL	4700	2.55	12500	12.00	9.107	3396	10	11278	13.35	10.26
ST. PAUL	BIT ZEIGLER No 1 ILL	250	4.70	12250	8.60	17.128	145	5	11303	15.65	6.54
" "	" " "	1200	4.70	12250	8.60	17.128	1201	8	11425	13.34	8.11
TOLEDO	BIT MILTON JACKSON LUMP	500	4.12	11475	6.91	16.029	395	7	11789	9.61	9.28
WASHINGTON	BIT SUGAR LOAF TWIN ROCKS PA	7000	3.55	14600	6.00	10.855	6267	10	14589	7.12	1.51
"	" " " "	500	3.98	14600	6.00	12.170	1124	5	14237	6.55	3.59
"	" " " "	350	4.03	14600	6.00	15.513	276	8	14371	6.79	2.65
"	ANTH No 2 BUCKWHEAT	2500	3.25	——	18.00	——	1196	5	11302	17.10	6.28
				DRY COAL							
CHICAGO	BIT PAWNEE HIMROD ILL	8000	2.90	13400	7.50		6242	19	11155	11.55	12.59
"	" " " "	500	2.90	13400	7.50		413	9	11310	11.30	11.89

ginia coals the range in moisture is from 1.51 to 4.45 per cent., the average being about 2.82 per cent. In the western coals, the moisture content is a still more important factor, as illustrated by the deliveries of coal of this class noted in Table I, reaching 12.59 per cent. in the case of the Illinois washed nut coal and nearly 8 per cent. in the Kentucky coal delivered in Louisville.

With respect to the ash, five out of the thirty-two contracts show a percentage superior to the standard fixed by the contractors and the variations are larger than in the case of the calorific value. A careful study of the monthly reports of analyses, which are too voluminous to be incorporated in this paper, indicates further that the monthly variations in ash percentage have been large, and that in the case of nearly all of the contracts one or more large deliveries of coal have been made closely conforming to the contract standard of ash. The occasional delivery of lots of coal high in ash has resulted, however, in materially raising the final average and has emphasized the necessity for system and vigilance in preparing and cleaning the coal as mined.

In one or two specific instances a wide departure from the specification standards leads to the inference that it was caused by the substitution of coal of different character than that upon which the contract was based. In either case, the advantage of an adjustable basis of payment for value received, such as is found in the specification referred to, is obvious.

On the whole, the experience of the current fiscal year is such as to inspire confidence on the part of the coal operator and dealer in the specification plan of purchase. No better proof of this fact can be offered than the results of the proposals which have been recently secured for the supply of coal for the federal buildings for the coming fiscal year. A complete summary of these proposals is given in Table II (pp. 586–588), which shows the delivery point, tonnage, description of character of coal desired, with accompanying maximum limits of ash, volatile matter and sulphur, all as specified by the government to guide bidders in making tenders on coal suitable for the local boiler and furnace equipment.

The data furnished by the bidders includes the commercial name of the coal, name and location of mine, designation of coal bed or seam, information as to sizing or screening, as well as the

TABLE II.—SCHEDULE OF PROPOSALS FOR COAL. U. S. TREASURY DEPARTMENT.
FISCAL YEAR 1909.

City	Tons 2240 Lbs	Description of Coal	Spec Limits Ash	Spec Limits Vol	Spec Limits Sulph	Bidder	Commercial Name of Coal	Mine	Vein or Seam	Screened Through	Screened Over	B.T.U. Coal as Received	Ash in Dry Coal per Cent	Price per Ton Bid	Price per Ton Adjusted to Lowest Ash Offered	Cost 1000 000 B.T.U. Comparative	Cost 1000 000 B.T.U. Actual	
																Cents	Cents	
Baltimore	2550	Coking run of mine	8	*22	1.5	A	Jenner	Jenner Somerset Co Pa	Upper Kittanning	—	—	14 250	7 50	$3.18	$3.20	10 025		
						B	Stineman	Stineman So Fork Pa	Miller	—	—	14 300	7.00	3 50	3 51	10.957		
						C	Imperial	Imperial Cambria Co Pa	Moshannon	—	—	14 000	7 00	3.20	3.21	10.236		
						D	Orenda	Orenda Boswell Pa	C Prime	—	—	13 500	7.00	3.29	3 30	10 913		
						E	Georges Cr	Washington No 2 & Borden Md	Pittsburg Seam	—	—	14 300	7.50	3.45	3 47	10 833		
						F	Cambria Pa	Vinton-Springfield Cambria Co	Miller	—	—	14 300	6.50	3 19	Std.	9.9588	9.9588	
						G	Sugar Loaf	Commercial 2 3 & 4 Expedit Cambria Co	Miller	—	—	14 300	7 00	3.31	3 336	10 414		*
Boston	1500	Coking run of mine	8	22	1.5	A	Beech Creek	Pardee Cambria Co Pa.	—	—	—	14 257	7 12	4 43	4.4702	13.997	13 872	*
						A	New River	Admiralty New R Dist	—	—	—	14 658	5 11	4 73	Std.	14 406		*
						B	Pocahontas	—	—	—	—	14 600	8 00	5.10	5.1578	15.771		
						C	New River	White Oak	—	—	—	14 500	8 00	4 95	5.1078	15 368		
	3000	Anthracite screenings	18	9	1	A	—	—	—	—	—	11 934	14 15	2.63	2 673	9.9992		
						B	—	—	—	—	—	12 650	12.00	2.80	Std.	9 8814	9.8814	
Buffalo	1250	Youghiogheny or equal	11	35	2.5	A	Shawmut	Shawmut-Elk & Jefferson Co Pa	Freeport		3/4" Bar	13 500	8 00	2 90	2.93	9.6892		*
						B	Webster	Ehrenfeld #3 Ehrenfeld Pa	Miller	Run of	Mine	14 279	6.87	3 65	3.6574	11 434		
						C	Reynoldsville	Florence & Eleanor	Lower Freeport S		3/4" Bar	14 000	6 50	3.00	Std	9 5663	9.5663	*
Chicago	8500	Illinois No 1 & 2 washed nut	10	38	2.5	A	Pawnee	Pawnee Vermillion Co Ill	—	No 1 & No 2 Mixed	2 1/2"	11 442	10.00	3 15	3.22	12.563	12.290	
						B	Shell Bark	Marion Williamson Co	No 7.	No. 5		11 646	8.60	3.25	3 2920	12.619		
						C	Indiana Nut	Enmandale Enmandale Ind	3rd Vein.	2 1/4"	7/8" R.O.	11481	16.60	3.36	3 5620	13 851		
						D	Illinois Nut	Lincoln Springfield Springfield Ill	3rd Vein	3"	1"	10 575	15.40	3.40	3 5380	14 936		
		New River Georges Cr &c	8	22	1.5	E	New River	White Oak McDonald Fayette Co Wva	Sewell S.		3" Bar	14 700	6 50	5.60	Std.	17.007		
Cincinnati	3000	New River or equal	8	22	1.5	A	New River	Oh Loup Creek Fayette Co Wva	Sewell S.	Run of	Mine	14 800	5.00	2.93	Std	8 8381	8.8381	
						B	Jenner	Jenner No 1 & 3 Somerset Co Pa	Upper Kittanning	" "	"	14 500	7.00	2.85	2.89	8 8978		
						C	New River	White Oak McDonald Fayette Co	Sewell S.	" "	"	14700	6 00	2.94	2.96	9.0102		
Detroit	1600	Coking run of mine	8	22	1.5	A	Pocahontas	Springton Mercer Co Wva	No 3	" "	"	15 000	5 36	3.65	3 6762	10 941		
						A	Kanawha	Berlin & Cumberland	Kanawha No 2	"	"	14 000	6.10	3 00	3 0410	9 6971	9.5663	
						B	Pocahontas	Tab Collieries McDowell Co WVa	—	" "	"	14 400	5 00	3 72	3.7390	11.591		
						C	Pocahontas	" "	—	" "	"	13 400	5.00	3 85	3.8690	11 994		
						C	Thacker	— —	—		"	14 400	5 00	3 35	3.3690	10.444		
						D	New River	White Oak McDonald Fayette Co	Sewell S	"	"	14 700	6 00	3.75	3 7890	11 507		
						D	Meadow Brook	Kanawha Co W Va on K & M Ry	—	" "	"	14 000	8 00	3. 20	3. 2790	10 456		
						E	Quemahoning	Jerome Somerset Co Pa	—		1" Bar	14 245	6.50	3 77	3.8190	11 969		
						E	Cabin Creek	Decota, W Va	—		1" Bar	13 800	7.50	3 24	3 3090	10.704		
						F	Georges Cr	Black Sheridan Wilson Co Md	—	Run of	Mine	14477	8 00	3.75	3 8290	11 807		
						G	Pocahontas	Welsh Mine WVa	—		1 1/4" Bar	14 277	4 05	3 58	Std	11 194		
Louisville	1850	Coking run of mine	15	42	2.5	A	Pittsburg	Becht Run Monongahela R	1st & 2nd Pool	3	3 Sq O	14 360	4 20	2.77	Std.	8.6115		
						B	Pittsburg	Camden Monongahela R	Thin Vein		1 1/4" Bar	14 000	6 00	2.65	2.686	8 5650	8.4503	*
						C	Fox Ridge	Coleman M Co Bell Co Ky	Kadwas Straight Cr		1 1/4" Bar	13 300	10.00	2 50	2.616	8.7809		
						D	St. Bernard	St. Bernard Hopkins Co Ky	No 9 Vein	—	—	11 500	13 50	2 25	2 436	9 4565		
						E	Cross Mt	Cross Mt Viva Ky	—	—	—	13 500	7 00	2 80	2.856	9.4444		
						F	Straight Cr	Straight Cr Pineville Ky		Run of	Mine	13 750	6. 00	3.20	3.236	10.5060		
						G	Regal	Southern Coal & Coke Campbell Co Tenn		No 2 Run	of Mine	13 000	5. 00	3.00	3.016	10 3520		
						H	Pittsburg	Risher Campbell Bell Bridge Pa		50% Lump	50% Nut	13 673	8 11	2.95	3 0282	9 8872		*

* Designates standard established by Geological Survey.

June '08.

J. E. Woodwell.

TABLE II (CONTINUED).

CITY	TONS 2240 LBS	DESCRIPTION OF COAL	SPEC LIMITS ASH	SPEC LIMITS VOL	SPEC LIMITS SULPH	BIDDER	COMMERCIAL NAME OF COAL	MINE	VEIN OR SEAM	SCREENED THROUGH	SCREENED OVER	B T U COAL AS RECEIVED	ASH IN DRY COAL PER CENT	PRICE PER TON BID	PRICE PER TON ADJUSTED TO LOWEST ASH OFFERED	COST 1000 000 B.T.U. COMPARATIVE	COST 1000 000 B.T.U. ACTUAL
														$	$	CENTS	CENTS
KANSAS CITY	2000	CHEROKEE SCR'ND LUMP OR EQUAL	15			A	CHEROKEE DEEP SHAFT LUMP	CHEROKEE SCRANTON CO. KANSAS	—		$1\frac{1}{4}$" R O	12 894	11 81	4 368	STD.	15.123	15.123
						B	CHEROKEE	CRAWFORD COUNTY ON M.C. Ry	FULLER NEAR PITTSBURG KAN		$\frac{3}{4}$ R O	12 000	12 00	4 10	4 1038	15.267	
MILWAUKEE	1200	YOUGHIOGHENY OR EQUAL	11	35	2 6	A	YOUGHIOGHENY SCREENINGS	MONONGAHELA R. CO. YOUGH. DIST. PA	2ND POOL	$\frac{3}{4}$ BAR		13 192	9 04	3 50	STD	11 844	
						B	YOUGHIOGHENY SCREENINGS	PITTSBURG COAL CO. YOUGH DIST PA	—		—	13 466	9 67	3 50	3 5126	11.645	11 603
MINNEAPOLIS	600	YOUGHIOGHENY OR ZEIGLER	11	35	2.5	A	POCAHONTAS	BERWIND W VA		1" BAR		13 800	9.00	4 75	4.794	15.509	
						A	"	" "		RUN OF	MINE	14 200	7.00	5 32	5.324	16.738	
						A	YOUGHIOGHENY LUMP	[illegible]	THIN VEIN		1" BAR	14 000	8 00	5.32	5.344	17.041	
						B	POLLARD	POLLARD MISS VALLEY WILLIAMSON CO ILL	—	6" or 3"	3" or $1\frac{1}{4}$ RO	12 250	9 00	4 55	4 594	16.742	
						C	ZENITH YOUGHIOGHENY	ELLA WESTMORELAND COUNTY PA	2ND POOL		2" BAR	13 380	6 80	5.70	STD	19.018	
						C	ZEIGLER	ZEIGLER NO 1 FRANKLIN CO ILL	—	6"	3" R O.	12 250	9 60	5.15	5.206	18 973	
						D	EMPIRE LUMP ILL	EMPIRE PEORIA COUNTY ILL	BED NO 5		2"	12 257	14 78	3 95	4 1096	14 968	14.387
						D	ZEIGLER	ZEIGLER FRANKLIN CO ILL	—	6	3"	12 257	8 40	4 80	4 832	17.574	
NEW ORLEANS	800	PRATT LUMP OR EQUAL	10	35	30	A	PRATT	MINERAL SPRINGS CR JEFFERSON CO ALA	—		2 BAR	14 000	6 00	[illegible]	[illegible] STD	[illegible]	[illegible]
	1300					B	PRATT	BLOSSBURG JEFFERSON CO ALA	PRATT		4" "	14 000	9 00	[illegible]	[illegible]	[illegible]	
						C	PRATT	PRATT PRATT CITY ALA	—		$1\frac{1}{4}$ R O	14 000	8 50	3 65	3 70	11 798	
NEW YORK *	2500	SCREENINGS RUN OF MINE	8	22	15	A	ACME BIT.	ACME HAWK RUN CLEARFIELD COUNTY PA	—	RUN OF	MINE	13 980	7 90	[illegible]	[illegible]	[illegible]	
	11000					B	WEBSTER	EHRENFELD NO 3 EHRENFELD PA	MILLER	" "	"	14 279	6 87	[illegible]	STD	[illegible]	
						C	CLEARFIELD	CLEARFIELD PA	—	" "	"	13 970	8 00	[illegible]	[illegible]	[illegible]	
						D	SOMERSET	PINE HILL [illegible] PA	—	"		14 000	8 00	[illegible]	[illegible]	[illegible]	[illegible]
	2500	ANTHRACITE SCREENINGS	18	5	1	C	ANTH SCR	PA				12 000	10 00	2 70	2 7932	10 392	10.045
						A	" "	"				12 000	13 34	2 82	STD	10 491	10 491
	4000	No 3 BUCKWHEAT ANTHRACITE	18	9	1	C	PITTSTON	PENN COAL CO PITTSTON PA				11 150	18 00	3.10	STD	12.412	12 412
BROOKLYN	3500	DITTO	18	9	1	C	"					11 150	18 00	3.38	STD	13 533	13 533
NEW YORK	500	No 1 PEA ANTH	16	9	1	C	MID VALLEY	LEHIGH VALLEY PA				12 181	14.89	4 46	STD	16.345	16 345
OMAHA	1700	CHEROKEE SCR'ND LUMP OR EQUAL	15			A	CHEROKEE	PITTSBURG CO. KAN. [illegible] KAN		RUN OF	MINE	12.000	11.00	4 41	4 47	16.630	16 406
						B	CHEROKEE	CRESCENT [illegible] WEIR KAN	WEIR CITY		$1\frac{1}{2}$ R O	12 000	13 00	5 24	5.34	19 866	
						C	BENTON	BENTON ILL		RUN OF	MINE	13 200	8 00	5 50	5 56	18 804	
						C	FLEMING	FLEMING KAN WEIR CITY		"	"	12 200	13.00	5.00	5.10	18.662	
						C	"	"			$1\frac{1}{2}$ R O	12 200	13.00	5.50	5.60	20 492	
ST. LOUIS	4500	CAR NO RUN OF MINE	12	03	3	A	RUBY	RUBY ST CLAIR COUNTY ILL			$1\frac{1}{2}$ BAR	10 500	12.00	2 04	2 1252	9 0149	8.6735
						B	STAUNTON	STAUNTON NO 7 STAUNTON ILL	No 6		3" R O	12 000	12 00	2 43	2 5152	9.3571	9 0402
						C	CANTINE	LUMAGHI, CANTINE ILL	GEOL No 6		3"	11 000	12.00	2 32	2 4052	9 7614	
						D	O FALLON	ST LOUIS & O FALLON NO 1 & 2 O FALLON ILL	BELLEVILLE		2"	11 000	16.75	2.19	2 3702	9 6193	
						B	GILLESPIE	GILLESPIE GILLESPIE ILL	NO. 6		$1\frac{1}{4}$ R O	12 061	7 74	2 53	STD	9 3646	
						E	B B LUMP.	SUPERIOR OR SUBURBAN ST CLAIR CO ILL		2"	$\frac{1}{8}$"	10 500	12 00	2 17	2 2552	9.5804	
						F	DONK	DONK BROS C & C CO MARYVILLE MADISON CO ILL			2 R O	11 000	12 00	2 35	2.4352	9 8831	
ST. PAUL	1450	YOUGHIOGHENY OR ZEIGLER	12	35	2.5	A	ZEIGLER	ZEIGLER FRANKLIN CO ILL		6" R O	3" R O	11 324	10 98	3 25	5.3096	20 932	
						A	YOUGHIOGHENY	ELLA WESTMORELAND COUNTY PA			2 BAR	13 180	8 62	5.87	5.8798	19 915	
						B	"	BUDD MINE THIN VEIN GAS C	2ND POOL		1" BAR	13 300	8 00	5 38	STD.	18 058	18.058

* DESIGNATES STANDARD ESTABLISHED BY THE GEOLOGICAL SURVEY

JUNE 08.

T. S. WOODWELL.

TABLE II (CONTINUED).

CITY	TONS 2240 LBS	DESCRIPTION OF COAL	SPEC LIMITS ASH	SPEC LIMITS VOL	SPEC LIMITS SULPH	BIDDER	COMMERCIAL NAME OF COAL	MINE	VEIN OR SEAM	SCREENED THROUGH	SCREENED OVER	B.T.U. COAL AS RECEIVED	ASH IN DRY COAL PER CENT	PRICE PER TON BID	PRICE PER TON ADJUSTED TO LOWEST ASH OFFERED	COST 1 000 000 B.T.U. COMPARATIVE	COST 1 000 000 B.T.U. ACTUAL
														$	$	CENTS	CENTS.
TOLEDO	500	NEW RIVER GEORGES CREEK	8	22	.15	A*	NEW RIVER	WHITE OAK MCDONALD MC FAYETTE CO WVA		3"	1¼" R.O.	14 700	7.00	4.25	4.3058	13.077	
						A	JACKSON HILL	SUN MINE NEW OF JACKSON OHIO		1¼"	1¼" R.O.	13 511	8.00	4.15	4.2258	13.963	
						B	NEW RIVER	MCDONALD LOUP CR FAYETTE CO W VA			3" BAR	14 500	6.50	4.30	4.3458	13.380	
						C	PENNSYLVANIA SMOKELESS	JEROME, JEROME SOMERSET CO PA	C PRIME		3" BAR	14 877	4.55	4.03	4.0368	12.113	
						C	BERWINDS SMOKELESS	1 2 & 3 BERWIND W VA. [illegible] W RY	POC. No 3.		4" BAR	14 997	4.21	4.26	STD.	12.681	
						C	NEW RIVER	MC DONALD CO. FAYETTE CO W VA			3" BAR	14 700	6.50	4.37	4.4158	13.411	
						C	POCAHONTAS	MILLER COALDALE CAN HORN & EMPIRE	POC No 3		3" BAR	15 000	4.67	4.48	4.4892	13.361	
						C	"	MERCER CO W VA DITTO.	" " "	2½"	1½" BAR	15 000	4.67	4.03	4.0392	12.022	
						D	CC & B POCAHONTAS	CC & B MCDOWELL COUNTY W VA	POC.	3" BAR		14 900	4.45	3.95	3.9548	11.849	11.834
PHILA	4000	CAKING RUN OF MINE	8	22	.15	A	HENRIETTE	HENRIETTE, [illegible] CAMBRIA CO PA	B OR MILLER	—	---	14500	6.00	3.60	3.62	11.145	
						B	WEBSTER	EHRENFELD No 3 [illegible] PA	MILLER			14279	6.87	3.35	3.3874	10.590	
						C	ACME	ACME, HAWK RUN CLEARFIELD CO PA				14000	7.80	3.14	3.1960	10.191	10.012
						D	MOSHANNON	MOSHANNON No 9 DRISCOL, CLEARFIELD CO	D VEIN			13915	6.00	3.24	3.26	10.450	
						E	OAKLAND	OAKLAND COALPORT CLEARFIELD CO PA				14400	7.50	3.50	3.55	11.062	
						F	EMPIRE	EMPIRE MINE CLEARFIELD CO PA	MILLER			14300	6.00	3.50	3.52	10.989	
						G	[illegible]	YORKSHIRE MADERA PA CLEARFIELD CO				14220	5.00	3.38	STD	10.611	
						H.	IMPERIAL	VIOLA, VIOLA, PA	D VEIN			13900	7.00	3.20	3.24	10.407	
						I	NANTYGLO	NANTYGLO CAMBRIA CO PA	B VEIN			14100	8.00	3.25	3.31	10.480	
						I	PENN No 8	[illegible] CAMBRIA CO PA	E VEIN			13700	8.00	3.19	3.25	10.591	
						I	STAR	TWIN ROCKS CAMBRIA CO PA	MILLER			14200	6.00	3.20	3.22	10.123	10.060
						J	PRISCILLA	PRISCILLA, SO FORK PA CAMBRIA CO				14500	6.50	3.98	4.01	12.346	
						J	CLEARFIELD	CLEARFIELD CO PA				13500	8.00	3.73	3.79	12.533	
						J	"	"				13500	8.00	3.23	3.29	10.879	WITH STRIKE CLAUSE
						J	PRISCILLA	PRISCILLA SO FORK PA CAMBRIA CO				14500	6.50	3.48	3.51	10.806	WITH STRIKE CLAUSE
						K	EMPIRE	EMPIRE, CAMBRIA CO PA				14300	6.00	3.50	3.52	10.964	
WASHINGTON	8000 2000					A	MUNNS	MUNNS HUNTINGTON W VA	FULTON & BARNETT			14 200	7.00	3.98	4.02	12.638	
						B	NEW RIVER	[illegible] FAYETTE [illegible]	SEWELL W VA			14 600	5.70	3.35 3.55	3.364 3.564	10.[illegible] 10.[illegible]	10.[illegible] 10.955
						C	IMPERIAL	IMPERIAL CLEARFIELD CO	MOSHANNON			13 900	7.00	[illegible] 3.55	[illegible]	10.591 11.530	
						D	CAMBRIA	SPRINGFIELD-[illegible] CAMBRIA CO PA	MILLER			14 400	7.00	3.34 3.56	3.30 3.60	10.479 11.[illegible]	
						E	NEW RIVER	EXPORT ROBINS FAYETTE CO W VA	FIRE CREEK			14 630	6.50	3.44 3.70	3.43 3.73	10.46[illegible] 11.382	
						F	SUGAR LOAF	TWIN ROCKS CAMBRIA CO PA				14 000	7.00	3.55 3.98	3.5[illegible] 4.02	11.[illegible] 12.8[illegible]	
						G	WEBSTER	WEBSTER EHRENFELD PA				14 279	6.87	3.55	3.5874	11.216	
						G	ADMIRALTY	NEW RIVER W VA				14 600	5.00	3.50	STD	10.742	

guaranteed British thermal units in the coal as delivered, and the percentage of ash in dry coal.

In order to determine a proper award of contract it is necessary to reduce the proposals to one common basis for comparison. This may be done in several ways, but the method chosen in this case is to first adjust all bids upon a given lot of coal to the basis of the same ash percentage by selecting that proposal which contains the lowest percentage of ash as the standard. Each per cent. of ash content is assumed to have a value of two cents per ton, the amount of penalty which is exacted under the contract requirements for one per cent. deficiency. The proposal prices have all been adjusted in this manner and are tabulated in Table II. Upon the basis of the adjusted price allowance is then made for the varying heating values by computing the cost of one million B. T. U. In this way the three variables of calorific value, percentage of ash and basic price per ton are all merged into one figure, the comparative cost of one million B. T. U., by which one bid may be readily compared with another.

For example, take two bids received for coal in Boston, Mass., as follows:

Bid A, New River coal,	14,658 B. T. U.,	5.11 per cent. ash,	$4.73 per ton.
Bid B, Pocahontas "	14,600 "	8.00 " "	5.10 "

The percentage of ash in bid A is then taken as a standard of comparison and the ash in bid B is 2.89 per cent. higher. Each per cent. of ash difference from the contract standard is rated at two cents difference in price per ton so that 2.89 per cent. of ash are valued at $.02 x 2.89 or $.0578 and bid B should be increased to $5.1578. The two bids are then on an equivalent basis so far as ash is concerned, as follows:

Bid A, 14,658 B. T. U.	$4.73 per ton.
Bid B, 14,600 B. T. U.	5.1578 "

The heating value being different, it is desirable to compute the cost of one million B. T. U. in each case by the formula:

$$\frac{1{,}000{,}000 \times \text{price per ton}}{2{,}240 \times \text{B. T. U.}}$$

For bid A

$$\frac{1{,}000{,}000 \times \$4.73}{2{,}240 \times 14{,}658} = \$.14406$$

and for bid B

$$\frac{1,000,000 \times \$5.1578}{2,240 \times 14,658} = \$.15771.$$

Bid B is therefore the most advantageous.

The necessity for having such a basis of comparison is evident from an examination of the bids for coal to be delivered in St. Louis as shown in Table II. In this case seven bids were received with guarantees of B. T. U. from 10,500 to 12,061 and of ash from 7.74 to 16.75 per cent., while the prices ranged from \$2.04 to \$2.35 per ton. Notwithstanding such apparent discrepancies, the comparative cost of one million B. T. U. ranged only from 9.0149 to 9.8831 cents.

Another interesting case appears as the result of the competition in Philadelphia in which sixteen proposals are based upon heating values ranging only from 13,500 to 14,500 and ash from 5 to 8 per cent. with prices more variable and ranging from \$3.14 to \$3.98 per ton. In this instance two proposals appear nearly equally advantageous, and to develop the relative suitability of the two coals for use in the down draft furnace with which the boilers are equipped, the practical performance of each kind of coal will be determined by actual trial of a test quantity of 50 or more tons.

In eight cases out of twenty-three the awards are made more advantageously to bidders other than the lowest, if price per ton was alone considered.

An examination of Table II should dispel any doubt as to the general applicability of the specification to a great variety of coal delivered in different localities under varying conditions. In the twenty-three cases low-grade anthracite, low and high volatile bituminous and semi-bituminous coals are represented, mined in ten states, Pennsylvania, Maryland, West Virginia, Ohio, Illinois, Indiana, Kentucky, Tennessee, Alabama and Kansas.

Coal from Pennsylvania, Kentucky and Tennessee competes in Louisville; from West Virgina, Maryland and Pennsylvania in Detroit, and from West Virginia, Ohio and Pennsylvania in Toledo, etc., and falls within the specification limits especially fixed in each case.

While a rating in comparative B. T. U. value enables the making of an award to the best economic advantage, in certain

instances other factors than the mere theoretical heating value may have considerable weight. This is especially so where any uncertainty exists as to the suitability or adaptability of an untried coal to the actual plant conditions of furnaces, grates, draft, labor of handling coal and ash, storage facilities, etc.

In some cases where boiler capacity is limited, grate area is small or draft weak, only the best grades of coal can be burned, such as are mined in the Pocahontas, New River or Georges Creek coal beds. In such instances it is desirable to take bids on a general specification so that the result of making radical changes in the boiler plant to take advantage of the coal which will give the best economic return may be determined.

In some cases it is well to test the market in more than one class of coal. In Chicago, for instance, two specifications were issued, one applying to the Illinois or Indiana washed nut coal and the other to the West Virginia or Maryland semi-bituminous coal. This action was taken in order to determine whether a change from the western coals high in moisture and ash and relatively low in B. T. U. value as well as price to the high-grade eastern coals at the higher price commanded in that locality would be justified.

It goes without saying that the relative facilities, competency and responsibility of the competing firms must also be recognized in making awards. A wide experience in the purchase of coal under various conditions has demonstrated that the application of the government specification removes much of the contention which exists when price alone per ton is the determining factor and the statements of bidders are unsupported by guarantees.

It is also believed that the prices at which coal is offered as shown in Table II are fully as low in most cases as the prevailing rates for coal sold under similar delivery conditions without guaranteed heating value.

The publication of authentic data relating to the quality of coal purchased during the current fiscal year as shown in Table II should result in a better understanding of the proper contract conditions in the making of future purchases.

The results of continuous inspection and analysis by the Geological Survey of deliveries of anthracite coal to government buildings in the City of Washington made during the current fiscal year

show almost without exception that a higher quality of coal has been delivered than during the preceding year. Though not all of the coal compared has been purchased upon a specification basis, the results warrant the conclusion that, directly or indirectly, the specification method applied by a number of the departments acted as a stimulus to the coal dealers and operators to secure the delivery of the best coal obtainable and the exercise of more care in its selection and preparation. The fact that a systematic inspection and analysis of all coal was being made has undoubtedly brought some protection to the government, even though the form of contract may not have provided in all cases as has been recommended, for adjusting the price paid to the quality of the coal actually delivered.

The government specification has also been applied to the purchase of coal for the navy yards at Charleston, Philadelphia, Norfolk and Pensacola, the cost of one million B. T. U. in the most advantageous bids being 12.41, 9.15, 8.62 and 10.16 cents respectively.

In one or two cases the interests of the government have been safeguarded by securing specific guarantees of calorific value. A contract for about 450,000 tons recently made by the Panama Railroad Company was based upon the delivery of coal of 14,600 B. T. U. per pound as received, at a price of $2.65 per long ton on board the consumers' lighters.

In conclusion, it seems wise to emphasize the importance of conducting all operations relating to the application of specifications, and the institution of methods of sampling, analysis and test in a manner which shall be worthy of the confidence of the coal operator or dealer. It is also evident that the best results will be secured only when such operations are conducted with competent technical or engineering advice.

TESTING LUBRICATING OILS.

By Henry Souther.

The automobile gas engine has presented new problems concerning lubricating oils. For high temperature work it might be assumed that high flash and high fire test oil would be most suitable, but this has proven to be an incorrect assumption. Such oils would clog the cylinder of an automobile engine at once. An oil leaving behind it no solid residue in the combustion chamber of the cylinders should be sought. To find a measure for this quality a new test has been developed that does not seem to be a correct one at first glance.

All automobile cylinder oils are distillates. They should not contain, therefore, solid coke-like carbonaceous matter. It has been found that they do leave behind a deposit of such matter, which raises the compression of the engine and becomes incandescent, causing pre-ignition under heavy duty, one of the worst of engine troubles. Heavy steam engine cylinder oils do this most quickly. This is not to be wondered at, as they are residue and not distillate oils. Some distillates also do it more quickly than others and the test which follows will indicate the relative quality of oils in this respect. Unfortunately this test carries with it a large element of personal equation. At the same time, closely comparable results are obtained by several operators in the same laboratory. An effort has been made in the following description of the method to make it possible for a reader to duplicate all conditions of manipulation.

Oils showing more than 0.50 per cent. of coke-like residue are not suitable for automobile engine work. The best of them contain 0.06 to 0.08 per cent.; a large number contain 0.20 to 0.40 per cent. and are entirely satisfactory. Steam engine cylinder oils contain in the neighborhood of 4.00 per cent. and cannot be used.

The other desirable characteristics are as follows:

Flash point (as determined in open cup), not less than 360° F.
Fire test, not less than 410° F.
Viscosity at 100° F., Saybolt viscosimeter, not over 300 secs.
Viscosity at 210° F., Saybolt viscosimeter, from 40 to 55 secs.
Viscosity at 210° F., Tagliabue viscosimeter, from 60 to 75 secs.

The flash point is the lowest temperature at which the vapor ignites and goes out.

The fire test is the lowest temperature at which the vapor ignites and continues to burn.

CARBON RESIDUE.

The apparatus used in this test is a glass retort of approximately 100 c.c. capacity, with a plain glass tubulature for a cork stopper. A ground-glass tubulature will not do, because heat will break it. These retorts are furnished by the Bausch and Lomb Optical Co., and known as No. 16,384 Special.

The method of operation is as follows:—Clean the retort carefully, and heat it over a Bunsen burner to remove moisture. When cool, suspend it from a balance by a light wire and weigh into it about 40 grams of oil. Distil over a Bunsen burner, with a cork stopper loosely fitted in tubulature. Commence distillation with the flame high enough to distil the 40 grams in about 30 minutes or at the rate of from 4 to 5 drops per minute. The rapidity of distillation can be determined from the rate of dropping of the oil.

A slight smoky vapor will accompany the distillate at the rate above mentioned. Toward the finish, during the removing of the last trace of liquid oil, the rapidity of dropping will diminish to a very few drops per minute.

Continue distillation until the residue in the bottom of the retort is nearly dry. Increase the Bunsen flame to its full extent and apply it all around the sides and neck of the retort until the oil condensed on the sides is also driven over. To accomplish this it will be necessary to blow the vapor out of the retort several times by removing the cork and blowing through the tubulature.

Then hold the retort over a blast lamp with a flame hot enough to wrinkle the glass but slightly. Continue this heating until all traces of oil are burned off, leaving only the carbon residue in the bottom of the retort.

The end-point of the determination is clear. All except the desired carbon residue will disappear very rapidly under this treatment. The carbon residue itself will begin to curl up if heated too strongly. The heating must be continued only long enough to dry the carbon residue proper. This is very easy if the

amount of residue is small, and requires strong heating. Heating must be stopped before the carbon begins to burn even on the edges.

When the retort is cool, weigh it again and determine the percentage of residue, as compared with the original weight of the oil.

When this crude idea was broached to me. I combated the possibility of there being any virtue in it. All those skilled in the art of testing also do. Now I am a convert in its favor, having learned by experience of a very practical kind that oil tested in this manner that gives high results also gives trouble in an automobile gas engine.

THE ANALYSIS OF OIL VARNISHES.

By Parker C. McIlhiney.

Of the materials used in construction few if any present more difficult problems in their testing and analysis than oil varnishes. Any rational system of testing varnishes to determine their suitability for a given use and their resistance to the destructive effects of exposure to the elements, will take account, among other tests, of a chemical analysis to determine the ingredients of the varnish and the proportions in which they are combined. It is unfortunate that there is not at the present time any method of analysis which will determine with any reasonable degree of accuracy, the proportions in which the oil, hard gum and common rosin have been combined to form the non-volatile base from which the varnish is produced by dilution with turpentine or other volatile oil. The proportion of volatile oil in the varnish may be determined by distilling the solvent off with steam at a temperature a little above the boiling point of water and then separating the volatile oil from the aqueous part of the distillate and weighing or measuring it. Its further examination need not be entered upon here since methods of analysis of such volatile oils as are likely to be used as thinners for varnish are now generally known and are described at length in such standard works as Allen's "Commercial Organic Analysis," and the books on paints and varnishes by Toch, Sabin, and Holley and Ladd.

The separation of the hard gum from the oil and the common rosin is the problem which is difficult; the hard gum and the oil do not unite at all, practically, until the hard gum has been melted and from 15 to 25 per cent. of its weight driven off as vapor, the amount so lost depending upon the character of the gum. After this melting the linseed oil may be added if it has been previously heated enough to prevent it from chilling the melted gum. This mixture of oil and gum is usually heated at this stage for a shorter or longer time to complete the combination of the ingredients. The union of oil and hard gum which has been effected by this means cannot be broken up by any solvent; or perhaps it would be more accurate to

say that after the combination between hard gum and oil has been successfully made, and the mixture thinned with turpentine, and stored for a few months, no solvent can be depended upon to regularly effect a separation of the two by its selective solvent action.

The process which is here described depends upon the fact that although the union between oil and hard gum is too intimate to be broken up by the selective solvent action of any solvent acting directly upon the original mixture, the combination may be broken up and the oil and gum brought back to more nearly their condition before they were melted together, by submitting the mixture to the action of caustic potash in alcoholic solution and subsequently acidifying the solution of potash salts so formed. By this means there is obtained from hard-gum varnishes a quantity of gum insoluble in petrolic ether very closely approximating the amount of hard gum actually existing in the varnishes, while the linseed oil is represented by its fatty acids which are readily soluble in this solvent unless they have been oxidized, in which case some of the fatty acids of the linseed oil will accompany the insoluble hard gum.

In carrying out the method an opportunity is given to determine not only the weight of the oil and of gum but also the Koettstorfer figure and the percentage of glycerin in the mixture. All these data taken together give a basis for corroborating the main figures.

The process is carried out by weighing into an Erlenmeyer flask 2 to 10 grams of the varnish, adding a considerable excess of approximately half normal solution of caustic soda or caustic potash in very strong or absolute alcohol, distilling off the major portion of the solvent, and redissolving in neutral absolute alcohol. The solution is then titrated with a solution of pure acetic acid in absolute alcohol, approximately half normal strength, to determine the amount of the excess of alkali present. From this the Koettstorfer figure is determined as the exact strengths of the acid and alkali solutions have been ascertained independently by comparison with known standards. A further quantity of the standard solution of acid in alcohol is added so as to exactly neutralize the total amount of alkali originally added. By this means the acid bodies liberated from their combinations with alkali are obtained in solution in strong alcohol. To this solution there is now added

a sufficient quantity of petrolic ether to dissolve the oil acids and this petrolic ether, being miscible with the strong alcohol, forms with it a homogeneous liquid. Water is now added to the mixture in such amount as to so dilute the alcohol contained that it is no longer a solvent for fatty or resin acids; this addition of water causes the petrolic ether which was mixed with the alcoholic liquid to separate, carrying with it the fatty acids. The rosin goes with the fatty acids while the hard gum, being insoluble in either the petrolic ether or in the very dilute alcohol, separates in the solid state. The aqueous and ethereal layers are now separated in a separating funnel and each is washed, the watery layer with petrolic ether and the petrolic ether layer with water. The petrolic ether layer is now transferred to a weighed flask, the solvent distilled off and the residue of fatty acids and common rosin weighed. This latter is then examined further by Twitchell's method to determine the amount of rosin which it contains, or it may be examined qualitatively in a number of ways to establish its identity.

The aqueous layer is freed from the suspended hard gum which it contains by filtering, and from any further quantity of gum which the weak alcohol may have retained in solution by evaporating off the alcohol and again filtering. The remaining aqueous liquid contains the glycerin and this is determined by the Hehner method with potassium bichromate—the method ordinarily used for examining spent soap lyes.

The hard gum is according to this plan precipitated in such a way that it adheres to the sides of the glass vessel in which the alcohol and petrolic ether mixture is diluted with water; the easiest method to weigh it is therefore to carry on the operation of dilution in a weighed glass vessel and then to dry and weigh the hard gum in this vessel. It frequently happens that some of the hard gum cannot be conveniently retained in this vessel but that it must be filtered out on a weighed filter and the weight so found added to that of the main portion.

If the varnish contains non-volatile petroleum or other unsaponifiable matter it will naturally be included in the fatty and resin acids, and it would be necessary to saponify the latter and extract the unsaponifiable matter from them while in the alkaline state; this operation is so familiar to chemists that it is mentioned here only to call attention to the necessity for it in some cases.

It would naturally be expected that on account of the well known insolubility of the oxidized fatty acids in petrolic ether, some of the acids of the linseed oil which had been polymerized by heat during the cooking of the varnish, or which had been oxidized during the blowing process to which some linseed oil is subjected before making it up into varnish, would fail to dissolve and would be counted in with the hard gum instead of with the linseed oil. It appears as a matter of fact that this source of error is of slight importance in the case of oil thickened by heat but that the blowing process gives an oil which is not completely accounted for by the soluble fatty acids recovered. This difficulty may be largely overcome by taking advantage of the greater solubility of the oxidized fatty acids in alcohol as compared with the hard gum; the freshly precipitated gum contaminated with oxidized fatty acids is treated with a moderate quantity of cold alcohol of about 85 per cent. and allowed to digest for some time. The soluble matter so extracted is then recovered separately by evaporating off the alcohol.

The great variety of hard gums in use and the great variety in the methods of making them up into varnish make the problem one of great complexity. It is not be expected that any one method of analysis or any single set of directions for carrying on the operation of making the analysis would be generally applicable, and it is not intended in this paper to give such detailed instructions. The method described has, however, been found to give upon samples of known composition made up under conditions which imitate closely the conditions of practice in an ordinary varnish factory, results that were accurate to within reasonable limits.

Rosin when present is usually combined with lime in the proportion of about 1 part of lime to 20 parts of rosin. An examination of the mineral constituents of the varnish is therefore of some value. The extraction of the mineral bases may be effected by treating a quantity of the varnish, somewhat thinned with benzine, with strong hydrochloric acid, and examining the aqueous liquid.

The amount of fatty acids obtained represents about 92.5 per cent. of the linseed oil. The identification of these fatty acids as belonging to linseed oil or to china wood oil may be satisfactorily accomplished in some cases, but there are undoubtedly many varnishes in which the analyst will be unable to identify and de-

termine the oils. The identification of the hard gums after separation from the other constituents of the varnish is a matter for which no rule can be given. The odor and physical characteristics of the recovered gum are quite as important as the known chemical tests of which the acidity and the Koettstorfer figure are among the most important. The chemistry of these gums is as yet almost unknown, but in the near future it is likely that our knowledge, both of the nature of these hard gums and of methods for separating them from the other ingredients of the varnishes of which they form a part, will be very greatly increased.

CERTAIN SOLUBILITY TESTS ON PROTECTIVE COATINGS.

By Gustave W. Thompson.

These tests are intended to be more for the purpose of suggestion than as giving conclusive results. It has been considered that, generally speaking, paint decay is largely due to the action of water and that this action is to a considerable extent a solvent action dissolving away the constituents of the paint. The method of testing used is as follows: The paint is coated on antiseptic gauze in the following manner: Strips of gauze 2 ins. wide and about 15 ins. long are dipped in the paint for about 13 ins. then drawn through a wringer consisting of two test tubes ($\frac{1}{2}$ x 6 ins.) drawn together by elastic bands. Gauze having these dimensions weighs about 1.25 grams. The weight of each piece of gauze is previously obtained and after it has been dipped in the paint, it is placed on a clock glass. The clock glass and its contents are then weighed from which, by deducting the weight of the clock glass and any adhering paint, we obtain the amount of paint which has been placed on the gauze. This gauze is then hung up to dry for about ten days with a tared clock glass under it, so that paint drippings may be caught and weighed and deducted from the amount of paint taken. After ten days the gauze and paint are weighed, and if desired the percentage gain in weight may be calculated. The painted gauze is then placed in a 150-c.c. beaker and covered with about 130 c.c. of distilled water. After 24 hours this water is poured off and evaporated and another 130 c.c. of water is placed on the gauze. The water poured off is placed in a tared vessel so that after evaporation to dryness the amount dissolved can be determined by weighing. This dissolved matter can then be analyzed for pigment constituents or treated with nitric acid and carefully ignited and weighed. The treatment with water can be repeated as often as may be desirable and, if it is thought best, the extract from several treatments can be combined and treated as one.

As an example of the results obtained following this method,

we give the figures, etc., obtained by treating eight paint mixtures in this way. The linseed oil used was two-thirds raw oil and one-third boiled oil—the boiled oil being a pure boiled oil made by the simple addition of oxides of lead and manganese at relatively high temperatures. The pigments and the approximate amount present were as follows:

(1) White lead	70 %
(2) Zinc oxide	66.6%
(3) Barytes	62 %
(4) Sublimed lead	71 %
(5) Fully hydrated sulphate of calcium	47 %
(6) Carbonate of calcium	50 %
(7) Red lead	80 %
(8) A mixture made by taking equal volumes of the (2) zinc oxide paint and the (6) carbonate of calcium paint containing 60% of pigment.	

The amounts of paint dissolved by the first three treatments combined and the fourth treatment weighed by itself are as follows:

No.	First Three Treatments Combined. Milligrams.	Fourth Treatment. Milligrams.
1	94	21
2	244	55
3	49	13
4	109	38
5	681	206
6	125	25
7	122	42
8	244	41

We give here also the amount of the principal constituents of the pigment found in the extract from the first three treatments and the amount of ash found from the fourth treatment—also the per cent. which this is of the pigment present.

No.		First Three Treatments. Milligrams.	Fourth Treatment. Milligrams.	Ash of Pigments.
1	Pb O	32	12	0.21%
2	Zn O	92	21	0.37%
3	Ba O	trace	2	0.06%
4	Lead	trace	—	—
	Zn O	36	15	0.22%
5	Ca So$_4$	510	159	8.52%
6	Ca O	26	6	0.32%
7	Pb O	69	27	0.34%
8	Zn O	85	15	0.39%

Blanks were of course run, taking two pieces of gauze from the same roll at opposite ends from where the pieces were taken in these tests. The results obtained show that the soluble matter from the gauze itself from three treatments amounted to about 2 mg.

We believe that these tests are instructive. For instance, it is found that by each treatment approximately 10 per cent. of the sulphate of calcium present was dissolved out. This is what we should expect but is not what many users of sulphate of calcium would be willing to admit takes place. As we have said, these tests are not conclusive. It would appear, however, that we may safely ask ourselves this question: Must there not be some other strong recommendation in favor of the use of sulphate of calcium in the preparation of outside paint to offset this very serious defect? The same question might be asked regarding mixtures of sublimed lead and carbonate of calcium, which may react upon each other to produce soluble calcium sulphate.

An examination of the solutions from the paints used before and during evaporation may disclose instructive information. Thus with these eight paints the following points were noticed:

The solutions from paints Nos. 1, 3, 4 and 7 were colorless before evaporation. All the others had more or less of a yellow color to about the following relative degree:

No.	2	1 degree.
"	4	$\frac{1}{4}$ "
"	5	$\frac{1}{4}$ "
"	6	2 degrees.
"	8	1 "

During evaporation the solutions from the zinc paints No. 2 and No. 8 clouded up with the apparent separation of zinc oxide. The other paints did not cloud up.

The condition of the paint after these treatments was in several cases curious and distinctive. Generally speaking, the lead paints were smooth and in very much the same condition as before treatment. The zinc paints were rough and puckered up showing numerous blisters filled with water. The calcium sulphate paint showed the loss of pigment. The calcium carbonate paint was much discolored and streaked yellow in places.

The solutions obtained from the first three treatments showed

unmistakable indications of the presence of manganese. This came from the linseed oil. It is possible that by prolonged treatment all of the manganese could be dissolved out of a dried paint, which would not be a disadvantage as its removal would eliminate a possible factor in future paint decay.

It should be noted that the zinc paint No. 2 and the zinc and calcium carbonate paint No. 8 gave on the first three treatments identical amounts of dissolved substances and very nearly identical amounts for the fourth treatment. No definite information is here obtained that there have been soluble compounds of the zinc oxide and the linseed oil formed. The writer has, however, found such compounds in other paints and believes that the formation of such compounds is largely the cause of the scaling of zinc. He is also inclined to believe that the tendency of galvanized iron to scale is due to similar causes. He feels that when the cure for one of these evils is found the cure for the other will also be found. He feels that by this system of testing paints a paint may some day be designed which will not scale on wood or galvanized iron. Incidentally he would remark that as a result of many tests he concludes that the scaling of galvanized iron is not prevented by washing with water, sapolio, ammonia, acetic acid or copper solutions.

In addition to the mineral matter dissolved from the paint by these tests a relatively large amount of organic matter was also dissolved. This organic matter on acidification with dilute HCl separated readily and consisted apparently of oxidized fatty acids, somewhat tarry in appearance and solid at ordinary temperatures.

As a broad proposition may we not safely conclude that, other things being equal, the protective coating which gives the lowest percentage of soluble substances is the best protective coating? May we not also conclude that since generally speaking soluble substances accelerate corrosion, the paint that contains the lowest percentage of soluble substance is to be preferred?

These tests should be carried further. They should be applied to such unoxidized compounds as lithopone and blue lead for the purpose of discovering whether they oxidize readily and, if so, whether soluble products are formed. They could probably be applied also to colored paints.

In conclusion: What we are most interested in at this time is not the results we have given here but rather the recognition of this test as a useful one to apply to protective coatings.

THE INHIBITIVE POWER OF CERTAIN PIGMENTS ON THE CORROSION OF IRON AND STEEL.

By Allerton S. Cushman.

In the discussion following Mr. Hughes's paper on "Deleterious Ingredients in Paint," presented before the Society at the last annual meeting, Mr. G. W. Thompson described a method of testing pigments to determine their relative electrolytic activity in relation to iron. Mr. Thompson also suggested that Committee U investigate this matter to see whether it can be determined if certain pigments have an inhibitive action on corrosion.

Considering the subject from a theoretical point of view and without any previous knowledge, prejudice, or opinion concerning any pigment whatsoever, Mr. Thompson's suggestion appeared to me to offer an attractive field for investigation which might possibly lead to results of great value. In view of the fact that past researches had shown that the soluble chromates exert a pacifying and protective effect on the surface of iron and steel, I made the following statement in my address to the Society a year ago:

"It would appear that slightly soluble chromates should be theoretically the best protectives for the first application to iron and steel surfaces."

On further consideration of the subject, however, it occurred to me that, if the chrome pigments had been precipitated in an acid medium or contained either by admixture or contamination soluble electrolytes which stimulated corrosion, in the ensuing struggle the iron might fare worse than if no especial preventive measures had been attempted. In order to get some information in regard to this point, samples of bright steel wire were immersed in 100 c.c. of a very dilute n/1000 normal solution of bichromate in a series of shallow dishes. The wire test pieces were suspended in the solution so that they did not come into direct contact with the glass surface of the dishes. This precaution should never be omitted in experiments of this kind, as, owing to the adsorption of air by glass, rusting is always stimulated at the point of contact between glass and iron. A dilute tenth-normal solution of copper

sulphate was now made up and used in the following manner: The first dish having been left as a blank, the second received one drop, equal to $\frac{1}{20}$ c.c. of copper sulphate solution. The third dish received two drops, and so on, each dish getting an increased amount of copper sulphate until twenty-five dishes had been prepared.

Now it is apparent that we have in this system two contending forces at work. Iron has a higher solution tension than copper and therefore tends to pass into solution, the copper tending to come out and deposit on the iron. Chromate ions on the other hand put the surface of iron in a condition in which it can not pass into solution. In the solution system iron-chromate-copper, we have an equilibrium to be decided between two contending forces acting in opposite directions. In view of this struggle which was known to be going on, although the actual conflict could not be watched, it was interesting and instructive to note the results. In the first dish, in which no copper was present, no corrosion took place, in the second also, no action was visible. In the third, however, minute specks of iron rust appeared. These were larger and more frequent in the immediately succeeding dishes, the test pieces showing rust tuberculation with the well known pitting effect. As the middle of the series of dishes was approached both iron rust and precipitated copper began to appear side by side on the surface of the iron and from there on in the series more and more copper separated, while less and less rust formed, until in the end dishes copper and iron were changing places evenly over the surface without apparent hindrance. These experiments and others of a similar nature were repeated many times with the same results and there seems to be no escape from at least the following two conclusions to which they obviously lead:

1. If the surface of iron is subjected to the action of two contending influences, one tending to stimulate corrosion and the other to inhibit it, the result will be a breaking down of the defensive action of the inhibitor at the weakest points, thus localizing the action and leading to pitting effects.

2. While the concentration of an inhibitor may be strong enough to prevent the electrolytic exchange between atom and ion, it must be still stronger to entirely prevent the solution of iron and the subsequent oxidation which leads to the formation of rust spots.

Applying these principles to the selection of inhibitive pigments it is apparent that many paints may contain substances well adapted for carrying on just such a struggle on the surface of iron. I take it for granted from the paint literature which I have studied that it is the concensus of opinion among experts that an ideal vehicle for spreading pigments does not exist. Linseed oil, well adapted as it is in some respects, not only undergoes complex changes but absorbs and carries water. Therefore, even when covered with the best paints the surface of iron is sooner or later subject to attack by the two factors which produce corrosion, viz., hydrogen ions and oxygen.

The method of testing proposed by Mr. Thompson can be used at least for dividing pigments into three main groups or classes: (1) those which stimulate corrosion, (2) those which are neutral, and (3) those which inhibit corrosion. The method of carrying on the test which we adopted was as follows:

A long train of bottles was arranged as is shown in Fig. 1, so that a blast of air could constantly be blown through under pressure.

A quantity of Bessemer steel sheet was obtained and used just as it came from the mill without preparation of the surface in any way. It was cut up into pieces of equal surface area, weighing about 4 grams. Into each bottle were placed two of these test pieces which had first been separately weighed to the nearest 0.0001 gram. 100 c.c. of laboratory distilled water was then added to each bottle, and equal parts by volume (10 c.c.) of the various dry pigments to be tested were added in succession to the bottles in the train. Blanks were inserted at the beginning, in the middle and at the end of the train. It was not considered necessary to determine the specific gravity of the pigments in order to get a precisely constant volume, as the roughness of the test renders such a refinement unnecessary, while the work and time required in such an undertaking would be tremendous where a large number of samples are to be tested. A small hollow brass cylinder with a handle soldered to it was used as a measure. This was heaped with the pigment, tapped three or four times and the mass smoothed off with the edge of a spatula. Thus in every bottle there was placed approximately the same volume of pigment.

The samples consisted of a collection of about fifty pigments

supplied by the Paint Manufacturers' Association of the United States, and represented the ordinary commercial pigments in common use. While the names and general composition of these samples was known to us, no effort was made to determine their degree of purity, or whether they contained soluble impurities which might stimulate corrosion. It was determined to let the test tell its own story, and then if necessary leave the interpretation to the future.

By means of the large pressure bottle at the beginning of the

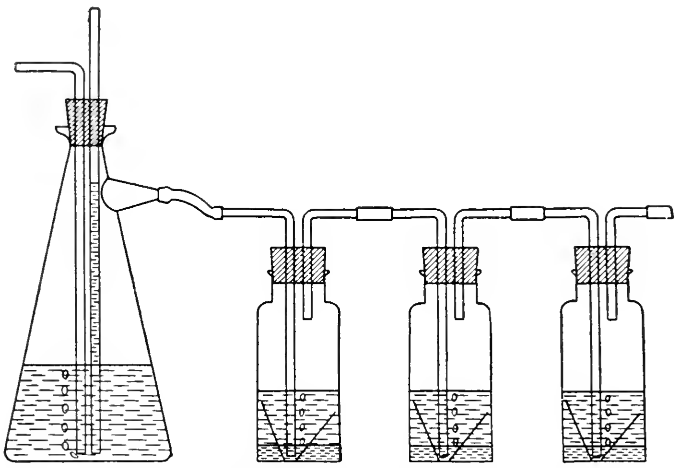


FIG. 1.—Thompson's Method of Testing the Inhibitive Value of Pigments.

train, a fairly constant rapid current of air was kept bubbling through.

The test was divided into three periods in the following manner: At the end of forty-two hours the samples were withdrawn, washed, brushed with a small nailbrush, dried, and weighed. They were then put back and the air blast turned on again for one week, after which they were again removed and weighed. Finally they were put back again, and the run continued two weeks longer. It would have been interesting to push the test still further, but lack of time and pressure of other work has not permitted this. The results obtained were plotted as actual

loss of weight, and were not calculated as per cent. of loss. Corrosion is entirely a function of the amount of surface exposed, and within the limits of the test has nothing to do with slight variations in the weight of the samples. The fact that each bottle contained two test pieces of equal area furnished a check on the work, and showed that the action going on was not haphazard. In every case the loss of weight of the twin samples was of the same order of magnitude, while in most cases the check was remarkably close. In plotting the results the total loss in the respective bottles was used.

The greatest perplexity which the results of this test engendered was to determine what use should be made of them. That they were instructive there seemed to be no doubt, but the fact that they appeared to condemn some types of pigments that have frequently been used and are highly thought of by many persons was somewhat alarming. Having in mind the fear that too hasty conclusions might be drawn which might prove harmful to certain interests, it was finally decided to reserve publication of all results until the work could be repeated by other investigators. More than fifty pigmentary substances were used and it was found that while the test pieces in some of the bottles were hardly touched, others were badly corroded and even pitted through. It we consider only the chrome pigments which were tested, it may be pointed out that a great difference exists in their inhibitive action. Some chrome compounds, among which zinc chromate is conspicuous, appear to be highly protective to steel in this test, whereas others show up as strong stimulators of corrosion. This variation in the action of pigments of the same general type is doubtless due to the presence of stimulative impurities. A test such as the one described, which will enable investigators to separate pigments into different classes as far as the resistance to corrosion which they offer, will therefore be of great value.

In conclusion let me call attention to a peculiar phenomenon which has been observed while carrying out these tests, and which I have discussed in a paper on "Electrolysis and Corrosion." In order to distinguish the two samples in each bottle from each other one was marked with a small dot which was prick-punched on the steel surface. In the cases in which the corrosion had gone far these small indentations were eaten through to pit holes.

This seems to show that if it is a matter of importance to avoid pitting, care should be taken to select steel which is as free as possible from indentations, scars, or wounds of any kind. Although it is not claimed that a true surface will not suffer at all, it is none the less true that each indented point will become the location of a destructive pit hole.

TESTING IS NOT INSPECTION.

By W. A. Aiken.

The original organization of the Department of Inspection of Material by the Chief Engineer of the Board of Rapid Transit Railroad Commissioners of the City of New York, through the appointment of a General Inspector of Material—a division engineer on his personal staff—recognized the department as an integral and coördinate part of the engineering force, to which was entrusted the design and construction of the New York subway, and sounded the keynote to which were attuned all subsequent efforts of the department to realize practically the theory of its service to obtain the best material possible under the specifications, thereby fulfilling its part in the trinity of essential entities of a complete engineering structure, where the best material is as necessary as the best design and the best constructive execution.

The specifications governing the work of the department had previously been submitted to the engineering representatives of prominent manufacturing interests, prospective bidders for furnishing the material and workmanship involved and had been readily accepted. Experience with these specifications throughout the construction period showed them to be normal and easily complied with by any modern and fairly equipped plant. Thus one vexed question, too often arising from contractors agreeing to specifications felt by them for one reason or another to be unnecessarily stringent, with the intention of subsequently requesting or forcing concessions, was wisely and happily eliminated. No valid objections having been found to the specifications, no valid objections could be urged against their enforcement by inspection.

If less attention were given to the individuality of specifications by the general adoption of standards—the promulgation of which, through interests representing both the consumer's and the manufacturer's points of view, is the policy of this Society—and if more attention were paid to the enforcement of any and all requirements of contract specifications, this would lessen the friction which at times arises from the above-referred-to temptation of the

framers of specifications to yield rights which had been previously acknowledged by the manufacturers' acceptance of specifications.

The president of the Society in his last annual address on the "Enforcement of Specifications," emphasized none too strongly the wisdom of his text, and showed the absolute necessity of insistence thereon, when material and workmanship were contracted for to meet specified requirements. He cited numerous examples from his extensive experience as chemist of the Pennsylvania Railroad of what could easily have amounted to practical nullification of contract, had not inspection, in its broader meaning, been insisted on.

It is not so much any failure of the engineering profession to recognize the intent of the inspection they call for, as it is the complete lack of detail acquaintance with what properly constitutes such service, that I wish to emphasize. The more intimate the acquaintance with, and the more thorough the knowledge of, the materials to be inspected throughout all stages of manufacture, certainly the more intelligent subsequent interpretation of the results of physical and chemical tests must be. Without this acquaintance and knowledge, through supervision of processes and absolute control of all test pieces or product samples, there is no inspection in its true meaning, and from absolute conviction on this material point came the primary differentiation, out of which grew the unsung slogan of the department, "Testing is not Inspection."

The writer, in concluding his paper before the Cement Section of the International Engineering Congress at the St. Louis World's Fair in 1904, where he had the honor of representing the American Society of Civil Engineers, made the plain statement that "Testing is not Inspection," no matter how conscientiously the former may be done.

When we examine commercial inspection practices—and note here, I do not refer to manufacturers' methods, except in so far as they are condoned by the purchaser's representative inspector, but rather to the latter's methods—we find they generally consist essentially in "testing" pure and simple. When the possibly unrepresentative character of the test pieces or product samples it realized, from the fact that it is the almost universal custom for the manufacturer to select these, as for example the case of struc-

tural steel, the fact stands forth prominently, that such "testing" amounts to absolutely nothing as "inspection," though possibly something as "insurance." In case of failure of any piece of construction, involving the question of quality of material, the purchaser could, under commercial service methods, swear in a court of law that he had taken the ordinarily accepted means to insure specification material. But what standing would such a defence have, if the prosecuting attorney showed that these ordinarily accepted means were radically ineffective in controlling desired results?

Real inspection furnishes insurance in its truest sense, just as good design and good constructive execution do, but this is merely the incidental and not the primary object of inspection any more than it is that of design or execution. The fundamental intent of inspection is to ensure material and workmanship in compliance with contract specification requirements, and this is not accomplished and cannot be guaranteed by mere "testing," no matter how conscientiously done.

Testing to be of the highest value and to fulfil its intended purpose must be more or less incidental to inspection in the generality of cases, and must be done from an engineering standpoint as opposed to a purely commercial one. Consequently, while its value and, indeed, to a considerable extent its absolute necessity, was fully recoginzed by provisions therefor in our specifications, it was more prominently recognized that unless test results were as theoretically intended, as absolutely representative as they could be made under existing conditions and through inspection, nothing could or, indeed, should be predicated therefrom. Hence, the first step was to ensure the representative character of test pieces or product samples by the selection by representatives of the department instead of by the manufacturer. Under ordinary conditions, there can be no *guarantee* that the test pieces or product samples selected by the manufacturer actually come from material they purport to represent. Undoubtedly they do in a large majority of cases, certainly, as a rule, it can be assumed that such is the intent; but inspection should not involve assumptions, unless absolutely unavoidable, and never when certainty can so easily be assured without infringing in the slightest degree on any one's prerogatives. Consequently, unless it is positively known

whence test pieces or product samples originate, results therefrom are certainly questionable if not worthless.

Again, unless test pieces or product samples represent as far as possible *all* the material of a certain lot, their value is measurably reduced. I have reference to the growing practice of steel mills rolling a limited amount of material from which test pieces are expected to be selected. Naturally greater care in the selection of such material, its heat treatment and mechanical manipulation, is to be expected under such conditions than in subsequent rollings of supposedly identical material, from which no test pieces are expected to be taken. I do not, of course, refer to the perfectly legitimate and proper practice of rolling preliminary tests for the manufacturer's own information; but no final inspection action should be taken on "sample test pieces" of any kind. All subsequent rollings should be subject to further test, if warranted, in the opinion of the purchaser's representative. When the manufacturer determines it to be unsafe to roll any considerable tonnage previous to formal test by the purchaser and prefers to submit "special" materials, the purchaser certainly has a perfect right to see that all subsequent rollings from this same melt of steel preserve, by equally careful manipulation, the special character given it by the manufacturer.

Again, the tonnage represented by any test pieces or product samples—irrespective of the previously discussed question, whether they were representative of *all* the material, i. e., of its average character—is one of vital importance and essentially a matter for inspection. Of what value is a test result positively known to represent a certain melt of steel, a certain quantity of cement, paint, asphalt or other material, if the invoiced weight or quantity of such material proves to be largely in excess of the unit represented, if the finished steel invoiced is largely in excess of the legitimate output from the furnace wherein the steel was melted, if the number of barrels of cement largely exceeds the capacity of the storage bin wherein the cement was ground when being sampled for test? Thousands of tons of steel are continually accepted under ordinary commerical inspection methods, entirely unrepresented by any tests, and impossible of identification, through lack of supervision of the legitimate percentage of finished material.

Record of all ingot or tapped weights in the case of steel, with

subsequent intermediate mill-process records therefrom to the finished material stage, should be at the disposal of the purchaser's representative. No hard-and-fast rule can or should be laid down as to the allowable percentage to be accepted. Average mill practice should ordinarily govern. Exceptional ingot output, or finished material weight from theoretical capacity, can easily be explained in individual cases by the mill records if the results are correct and no "mix-up" has occurred, as at times accidentally happens; but this latter condition, if inexplicable from the records, should condemn all the material involved, since in such cases tests mean nothing.

Again, the question of section and weight ordered is an important one. Apart from the money involved, sections called for are required to satisfy stress conditions in the structure. If light sections are furnished and accepted, as is too often the case, the factor of safety is lowered. Paradoxical as it may appear in these "tonnage" days, when everything animate and inanimate is strained to the breaking point to exceed "last month's output" and win the "broom" pennant, with the increasing practice of salary bonuses on output as further incentive, our experience has been that constant vigilance is necessary to obtain always full sections.

Again, in the matter of limiting chemical requirements in specifications, I am satisfied that little if any attention is paid to these under ordinary commercial inspection methods. Unless the specifications are clear in the matter, of course a perfectly legitimate difference of opinion may properly arise as to where the drillings in the case of steel, on which check analyses are to be made, are to be taken, from the pouring ladle or finished material. It is naturally the intent of engineers in prescribing limits, to cover the character of material actually used in the construction, i. e., the finished product; but it has occurred in my experience that a manufacturer attempted to prevent check analyses of material at any stage of manufacture, because the specifications, while limiting certain elements, did not categorically and specifically provide for check determinations. Under commercial inspection practice, the purchaser simply accepts a record of the manufacturer's analyses. Without the slightest reflection the case is similar to that of test pieces of the manufacturer's selection, the results being

equally valueless in my opinion. However, if the selection of the test pieces is left to the manufacturer, it is perfectly logical to accept his reported chemical determinations, but to do so is to waive inspection in either case.

Many other points involved in the differentiation between testing and inspection could be cited if time permitted, but the crux of the subject is comprised in the statement that too much cannot be known about material to be inspected and that in proportion to the more intimate acquaintance of the inspector with the material, the deductions or conclusions from the tests become more valuable. A fuller recognition generally of what inspection really means and an appreciation of the differentiation made in this paper would undoubtedly tend to the betterment of such services .

Commercialism has been the bane of most so-called inspection in this country, and the sooner the quality of service, rather than its cost is recognized, the sooner will this important engineering adjunct receive the appreciation it deserves. If inspection is valuable in practice—and no one will deny its worth in theory—cost *per se* should never enter into consideration. The character of service obtained under the writer's supervision during the past eight years, where every result was representative, has satisfactorily established the fact that inspection can be done as it should be done, but seldom is done, at a reasonable cost.

DISCUSSION.

The President.

THE PRESIDENT.—I would like to say, simply as a matter of history, that I remember very clearly the day when first in the Pennsylvania Railroad specifications it was decided that before the specification was issued it should be submitted to the manufacturers. Like everybody else, at that time we had been accustomed to say that it is the duty of the engineer to make the specification and the duty of the producer to fill it. Our experience, however, with a few specifications led us, to the knowledge that there was a good deal of dissatisfaction on the part of the producers after the specification was issued. Finally at a conference in Altoona between the engineer of tests, the chemist and general superintendent of motive power over a new specification, I proposed that before the specification was issued it should go to the manufacturer and be criticized by him. So the proposed specification was sent to the purchasing agent with the request that he send it to producers and ask them for their criticisms: "Can you make the material? Are the requirements reasonable? Is there anything objectionable in that specification?" The first answers we got were occasionally ludicrous. Sometimes they consisted of two lines. "We can make this material if anybody can." Apparently the producers were afraid and didn't quite dare to say what they thought, because "here is a party who controls orders and maybe if we are a little too strenuous in objecting to their specifications they will send their orders somewhere else." So, finally, a second call was sent out: "Don't be afraid to strike square from the shoulder. Say everything you want to say and fight just as hard as you can." That practice has been in vogue in Altoona for twenty years. No specification is issued that does not first go to the manufacturers, to be criticized by them; and gradually they have recovered their courage and they strike back, and that is just what is wanted before the specification is issued.

Mr. Thompson.

MR. G. W. THOMPSON.—One important thought which Mr. Aiken brings to my mind, and with which I heartly accord, is that the consumer, the person who is furnishing the material under

Mr. Thompson. contract and specification, should be put in a position to obtain all possible knowledge in regard to materials that are furnished. Analysis alone will not furnish it. He should be given all possible knowledge, given every opportunity to obtain knowledge. What we want is the knowledge; and to shut off any one avenue of knowledge is to shut off one of the avenues of progress.

Mr. Lesley. MR. R. W. LESLEY.—Having a little experience as a pioneer in the field of thought opened by Mr. Aiken's statement that inspection is not ordinary testing, I may, from the manufacturers' standpoint, throw a little light on the subject. In the case of a large contract some years ago where ordinary methods of inspecting cement at the time of its arrival on the work would have involved the cluttering up of the streets of New York with many hundreds of thousands of barrels of cement, pending the 7 and 28 day periods for acceptance, the outlook was that it would be impossible to have a proper inspection without a popular clamor against the impediments on the highways. For the first time the cement manufacturer came to the front and said, "we will permit you to inspect this material at our mills and not only give you cement to meet the ordinary methods of test, but will furnish you a house for a laboratory of your own; we will furnish heat, light, water, electrical apparatus; we will furnish you testing apparatus; will give you the necessary chemical laboratory and will give your inspectors the run of our mills in order that they may see the methods of manufacture and inspect as well as test."

In that industry I suppose I was a sort of iconoclast at that time, because up to that period we who had been pioneers in the cement industry had kept things rather quiet and dark, and did not allow people to come prowling around our mills. As a result of this forward step in inviting inspection as well as testing, we have had during the past few years less criticism upon our cement, though the capacity of our mills has been doubled, fewer complaints and more scientific knowledge and thought in the manufacture of our product. By reason of this new method we have had not only testing, but we have had inspection. In fact we have had a most critical examination of our material at every stage of its manufacture.

This precedent of testing at the mill has been followed in the cases of many contracts so that at our plants we have had at all

times from fifteen to twenty inspectors examining our product, with the freest access to all parts of our plant and with the heartiest coöperation on the part of our manufacturing department. While, possibly, unnecessary criticisms may have been made, other criticisms of value have led to important improvements, and on the whole the manufacturer may well say that while the old-time testing was not inspection, the modern inspection is of the best class, and the more of it we get the better results will be had for the community at large. Mr. Lesley.

NOTES ON THE HISTORY OF TESTING MACHINES WITH SPECIAL REFERENCE TO EUROPEAN PRACTICE.

By J. H. Wicksteed.

The history of testing by means of testing machines probably goes no further back than the history of chain cables; for although experiments on the strength of materials were made earlier, yet it is pretty certain that all those experiments were made with direct loads and the specimens when of iron were reduced to sufficiently small dimensions to make such direct testing applicable, although no doubt it was laborious. But when chain cables for ships began to come into use, as it is obvious that a chain can not be reduced like a sample of iron, to suit the power of the testing appliances, a testing machine had to be made to prove such an article, and a machine for the purpose was made in 1812, and was probably the first veritable testing machine ever made.

It will probably be a surprise to many to hear that the use of chain cables is of so recent a date, since the Romans are reputed to have used them for securing their galleys, and it is on record that in 1690 Sir Cloudesley Shovel recommended that chain moorings be introduced into the English service; but the ground moorings of Sir Cloudesley Shovel were not much like the modern short link chain, and the Roman moorings, according to Vegitius, were cast of brass. But in 1808 Robert Flinn, an Irish smith at North Shields, made a chain cable, and in the same year Captain Samuel Brown, R.E., represented to the naval boards the policy of employing iron rigging and chain cables, and on February 4 of that year, he enrolled a patent for those articles. Shortly afterwards he fitted the Penelope with them, made a voyage to the West Indies, and on the return of that ship in 1810, he represented the practical advantages which he found in iron rigging, but more particularly in the cables, which were in all respects superior to those of hemp.

In 1812, Captain Brown improved his chain by scarf welding at the sides, and in the same year he introduced a machine "which, by enabling him to put as great a strain upon the cables as was likely

ever to be brought upon them when in use, thus detected any defective materials or insufficient workmanship." This, then, was the beginning of testing machines, and the firm of Brown Lennox is still in possession of the original sketches made by Captain Brown of his machine.

It was a 100-ton machine consisting of a long bed, at one end of which was a powerful double-geared crab turning a chain barrel. Two side link chains wound round the barrel, stapled to the two outer ends and converging as they coiled up towards the center of the barrel, thus uniting to give a central pull on the shackle to which the test chain was attached. At the other end of the bed there were a pair of compound levers having a ratio of 200 to 1. The levers had knife-edged main fulcrums, but instead of having knife-edged load fulcrums, they had rounded butts on which side link chains were fixed and from which they pulled at tangents, so that the fulcrum distances were the distances from the center lines of these chains to the knife-edged fulcrums. The upper lever carried a scale pan for weights.

In 1813, Mr. Brunton patented studded links, and had a machine erected at his factory in the City Road which consisted of a long bed at one end of which was a fixed point of attachment for the chain and at the other end a Bramah's press. The chain was stretched between the two and its strength estimated by a small hydraulic valve on which every pound weight answered 16,000 times that amount of pull by the press. It had no weighing levers and trusted for its indications to the very small hydraulic valve.

The result given by this machine as compared with Captain Brown's differed in the proportion of 50 to 59, so that when, in the year 1831, the vessels of the royal navy were to be supplied with chain cables on a more extended scale, the admiralty, unwilling to trust to these uncertain measures of the strength of their cables, determined to build a machine which should combine both these means of estimation, giving more particular attention to perfecting the lever system, since Captain Brown's machine was considered the more reliable of the two. Accordingly they ordered their first chain testing machine from the firm of Messrs. Bramah of Pimlico. It was to combine the principle of both Brown's and Brunton's machines, the hydraulic press being placed at one end of the machine bed to strain the chain, and the steelyard system at

the other end to measure the stress. The levers had knife-edged load fulcrums as well as knife-edged main fulcrums and the leverage was 2,240 to 1, so that each 1 lb. weight in the scale pan balanced 1 ton of pull upon the chain cable. The machine was finished in 1832 and was started in regular work at Woolwich Dockyard in April, 1833.

Professor Barlow made experiments on the strength of materials in this machine and very inconvenient he must have found it for his purpose. It was suitable enough for testing 70- or 80-ft. lengths of chains up to a proof load which would lift the scale pan, but for testing short samples, the coupling up and balancing must have required the expenditure of much time and patience.

It was not until 1852 that a good laboratory machine was made for testing in compression as well as in tension. In that year, Ludwig Werder of Nürnberg made a horizontal machine in which the hydraulic press for straining was at the same end of the gauntry as the lever for measuring the stress. The specimen was strained through the weighing lever which was mounted on the hydraulic ram, so that when the ram traveled out, the lever traveled with it, dragging after it the frame which both strained the specimen and weighed the pull. The other end of the specimen in tension tests was fixed to a bulkhead blocked out upon the gauntry; in compression tests it abutted directly against the solid end of the hydraulic cylinder. In this machine the pull was balanced by weights put into a scale pan, and the leverage was 500 to 1.

In all the machines hitherto mentioned, detached weights were applied in a scale pan to balance the pull, and this intermittent balancing rendered these machines unfit for continuous or rapid testing.

In 1863, Mr. David Kirkaldy made a machine in which the steelyard carried a traveling poise by which he could continuously balance the stresses. He also introduced four screws for adjusting a straining cross-head at a variable distance from the levers so that the machine could take long or short test pieces. His hydraulic cylinder was placed at the opposite end of the gauntry to the levers and he had removable stirrups for compression tests. Further, he fitted his machine with a removable beam for cross bending, a removable apparatus for making torsion tests, and one for making shearing tests in double shear.

The leverage ratios in Kirkaldy's machine were about 130 to 1 up to 50-ton loads, and about 1,500 to 1 up to 500-ton loads, there being two steelyards, one with double and the other with treble purchase.

In 1879 machine built by Mr. A. H. Emery was started at Watertown Arsenal, U. S. A., which would test in all the senses of Kirkaldy's, to about the same loads, and which would take columns 30 ft. long and 2 ft. 6 ins. by 2 ft. 6 ins. in cross-section. The nominal capacity of this machine is 357 imperial tons.

At Charlottenburg, a machine was built in 1894 to test up to 492 imperial tons, and to take in columns 49 ft. long and 2 ft. 7 ins. by 2 ft. 7 ins. in cross-section.

In review of these machines it may be said that in the Werder machine the balancing levers are carried on a hydraulic ram, and pull the straining cross-head with them. The rest of the machine consists of an inert-bed, extending in the opposite direction to the ram, and on this bed short lengths of cast iron pieces are placed so as to vary the distance of a stationary cross-head to suit long or short tension pieces. For compression tests the distance from the cross-head carried by the balance to the butt end of the hydraulic cylinder is altered by means of links, so that longer or shorter pieces may be compressed between the two.

In the Kirkaldy machine the hydraulic cylinder is in the same position as Werder's with the ram working outwards from the end of the bed, but the balancing levers are at the other end of the inert bed. The ram pulls the straining cross-head through four screws, and this cross-head can therefore be adjusted to any required distance from the balancing cross-head, beyond that side of the straining cross-head which is opposite to the hydraulic cylinder.

In the Emery machine the difficulty of adding or removing parts to change the machine between tension and compression is avoided by having the hydraulic cylinder double-acting, so that it will either push or pull on the balance, and by making the balance so that it can either be pulled against a chamber, containing fluid, which delivers the pull to the scale beams, or the chamber can be pushed against the balance, which likewise delivers the pressure to the scale beams. The difficulty of coupling the machine to long or short pieces is met by moving the hydraulic

cylinder, with its double-acting piston and straining head complete, to the desired distance from the balance.

In the Hoppe machine at Charlottenburg, instead of having a double-acting piston, as in the Emery machine, the hydraulic cylinder can be locked to the bed so that the ram goes forward, or the ram can be locked to the bed so that the cylinder goes backward. The straining cylinder will therefore exert either a push or a pull, and the balance is made so that it will deliver either a push or a pull to the weighing levers, by arranging it so that a push acts upon the short arms of two elbow levers, while the elbows form the fulcrums and a pull acts upon the elbows of the levers, while ends of the short arms form the fulcrums. In other words, the elbow levers, when balancing tension, act as levers of the second order, and, when balancing compression, as levers of the first order. The difficulty of coupling to long or short pieces is dealt with, as in the Emery machine, by moving the hydraulic cylinder upon the bed towards or from the weigh-bridge.

In 1904, a machine built by Joshua Buckton and Company, Ltd., of Leeds, was started at the Conservatorie des Arts et Metiers, to test in compression, tension, deflection, and shearing. It takes in columns 88 ft. long in compression, chain cables 13 fathoms long in tension, beams 20 ft. between supports, 6 ft. 8 ins. deep, and 3 ft. 3 ins. broad, in cross bending, and it carries a shearing apparatus which will test in single shear a bar 8 ins. wide and $2\frac{1}{2}$ ins. thick. This machine will admit a stanchion 88 ft. long, 40 ins. wide, and 40 ins. deep, with a baseplate 20 ft. long and 40 ins. wide, strutted 6 ft. 8 ins. high against the vertical member. Whatever may be the kind of test, the machine is capable of tracing a complete diagram. The loads are recorded upon a large drum by a vertical displacement of a pen by means of a wire which moves an amount proportionate to the displacement of the traveling poise weight. By means of a train of chain wheels, actuated by the hand wheel controlling the traverse of the poise weight the motion of the pen can be regulated at will to give a scale of ordinates of 10 mm., 5 mm., 2 mm., or 1 mm. per ton. The deformation of the test piece is transmitted as abscissas to the recording drum by suitable wires passing over pulleys which turn the drum in such a manner as to record the deformation either full size or multiplied five or ten times.

In the Buckton machine, the hydraulic ram moves a frame which is as long as the longest specimen. This straining frame is both surrounded and traversed by a balancing frame The result is that the straining cross-head can push against one part of the balancing frame and pull from another part, and can itself be run along the straining frame into a position for pulling or pushing either long or short pieces.

Finally, the author's experience in testing both materials and members of structures, leads him to place more reliance upon the stress-strain curve drawn by a first-class autographic recorder than upon any number of separate observations taken at intervals during the progress of a test.

SPECIAL FEATURES OF A RECENTLY INSTALLED 600,000-LB. UNIVERSAL TESTING MACHINE.

By Thorsten Y. Olsen.

Testing to-day has assumed proportions far greater than in the past. Methods of construction have changed, due largely to the introduction of reinforced concrete. Large beams and columns are being used in building construction which cannot be tested owing to the lack of large testing machines. Within the past few years the demand for testing full-size members has greatly increased. I wish to describe a structural materials testing machine of 600,000 lbs. capacity which has not been exceeded to the present date in machines of the screw type. Two such machines have recently been completed and a third is in course of construction.

One of these machines has been in operation at the structural materials testing laboratory of the U. S. Geological Survey at St. Louis, Mo., for the past eight months and another, of somewhat different general dimensions, has recently been installed in the Civil Engineering Department of the University of Pennsylvania. A third machine, a duplicate of the latter, is nearing completion for the Rensselaer Polytechnic Institute, Troy, N. Y.

Some of the principal points in the specifications for these machines which define the type of construction and general dimensions are as follows:

1. Provision of guide columns for traveling cross-head.

2. Device to absorb the recoil after rupture of specimens under heavy loading and to protect lever system.

3. The adaptation for the University of Pennsylvania machine is 22 ft., with 20 per cent. elongation, in tension, 24 ft. in compression and 20 ft. transversely in bending.

The adaptation for the structural materials testing laboratory machine is 24 ft. with 25 per cent. elongation, in tension, 30 ft. in compression and 25 ft. transversely in bending.

To better describe the special features of this machine I shall cover briefly the whole construction dwelling more in detail on the important points.

The machine will be described under the following headings:

Straining mechanism.
Weighing mechanism and extensions for beam tests.
Traveling cross-head and guide columns.
Method of absorbing recoil.
Tools for operation.
Method of control and use of machine.
Calibration and acceptance tests.

Straining Mechanism.—The straining mechanism consists primarily of four main screws A–A, Fig. 1, drawn directly down without rotation by means of four large gear-nuts B–B. The screws are kept from rotating by means of four long feathers at C in base cover and at D in the lower cross-frame in the base. These four gears mesh into the large center pinion E, which, through a bevel-gear and pinion, connect to the outside main gear F. This in turn is driven by a 15 H.P. motor through a series of spur gears to obtain the desired speed changes.

The base of the machine consists of four parts bolted together as shown and braced with a tie bar to produce the rigidity required. Provision is made on the side plate for the support of the recoil cylinders and the sides are here strengthened to withstand the pressure due to the action of these cylinders.

Only cut gears are used on this machine, and all main gears are of cast steel. Steel gears B–B have bearing nuts securely keyed to them which afford a total longitudinal bearing length on the screws of 78 ins.

Ball thrust bearings are inserted between gears and base cover, and the pressure on the balls is equalized by rubber cushions. Provision is made for taking up wear on the bearings and for renewing them without dismantling the machine.

Speed changes are taken care of by change gears controlled by a clutch at L, slow speed reversing gears at the end of the counter-shaft, and a speed regulator in connection with a 4–1 ratio electric motor. The motor is direct connected to the frame, and a silent chain drive is used to transmit the power. A chain drive

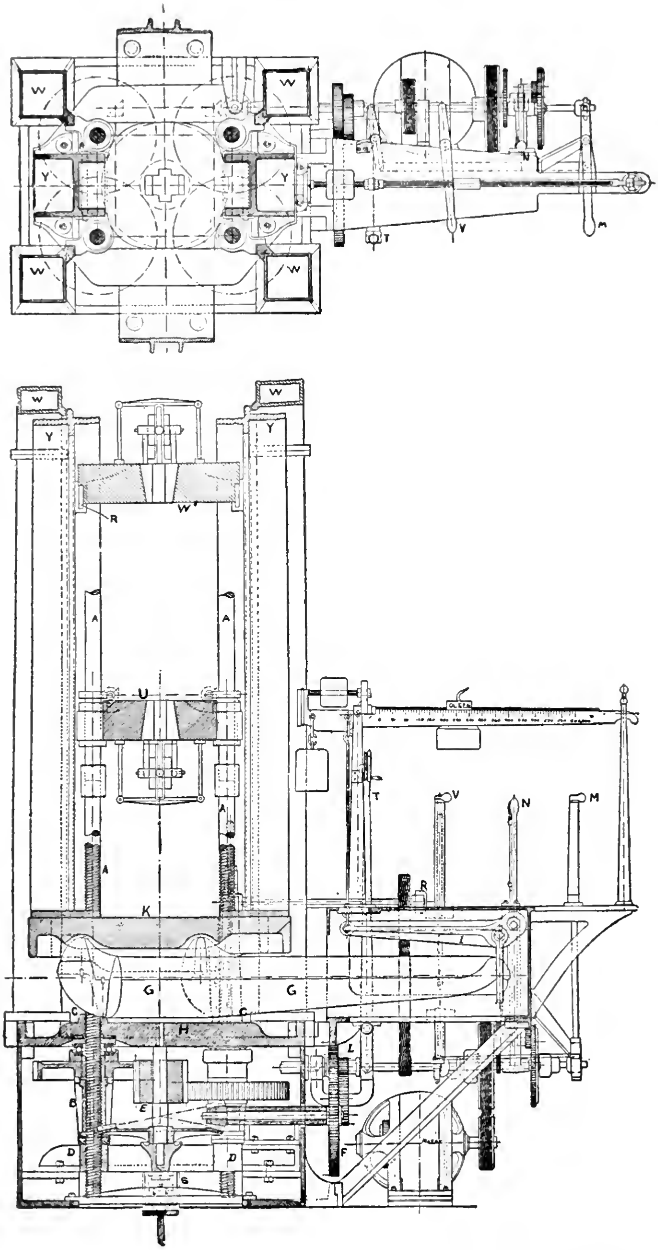


FIG. 1.—Details of Olsen's 600,000-lb. Universal Testing Machine.

is also used to transmit the power from the main countershaft to the shaft at R operating traveling cross-heads to be described later.

Weighing Mechanism and Extensions for Beam Tests.—The weighing system consists of a series of three levers G, acting as one lever between the base cover H and the weighing table K, and transmitting the load through the intermediate lever I to the scale beam where the pressure is weighed.

The scale is made in three types as follows: Dial-vernier screw beam, as on the University of Pennsylvania machine; automatic beam, as on the Rensselaer machine; and automatic and autographic beam as on the structural materials testing laboratory machine. These beams are all provided with three poises, reading to maxima of 600,000 lbs., 300,000 lbs. and 30,000 lbs. respectively.

The autographic beam is a very accurate device for determining the stress-strain diagram. The ease and rapidity with which it may be connected to the specimen and operated makes it a desirable attachment for commercial testing as well as scientific research. Space does not permit of its detailed description.

In the weighing mechanism all the levers as well as the beam extensions connected to the weighing table and the cross-heads U and W′ are of cast steel. The weighing table and tension columns Y–Y as well as the rest of the supporting parts are of cast iron.

The beam extensions are bolted directly to the weighing table and braced by means of struts, tension rods, and a connecting chord so as to make a rigid structure capable of supporting a beam of 20 ft. length transversely under a maximum center load of 200,000 lbs.

Traveling Cross-Head and Guide Columns.—The tension columns Y–Y which support the upper cross-head are built in three sections and have pockets at intervals as shown at R′ in which cross bars may be inserted to hold the upper cross-head W′ at whatever position may be desired, depending upon the length of specimen to be tested. These columns are connected at the top by means of a cast steel tie plate and are also held in proper relations to the guide columns, to be described later, by a series of flexible plates, both at the top and at the middle of the columns.

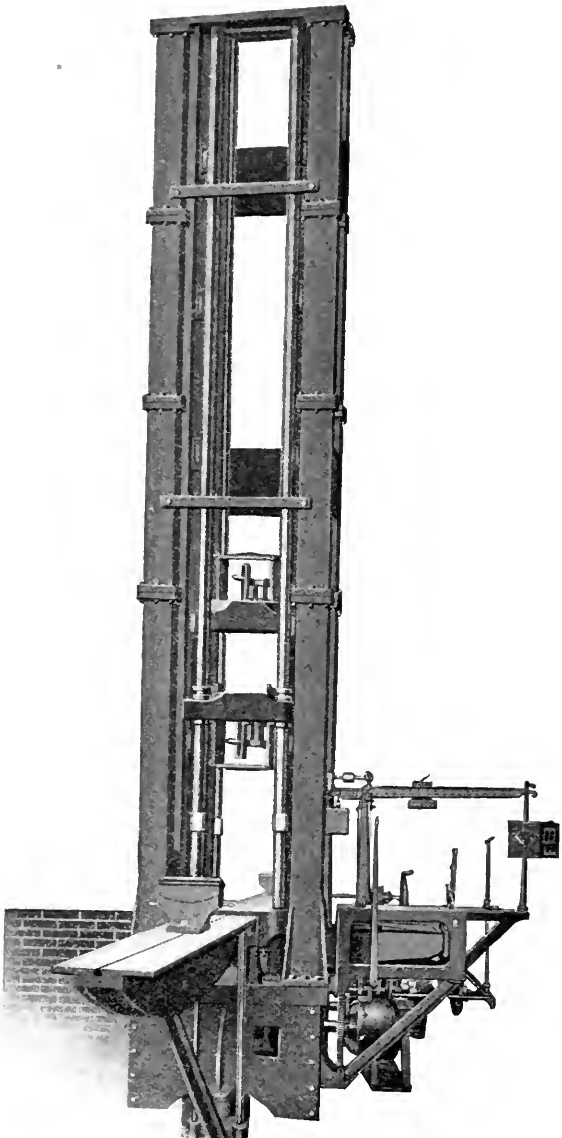


FIG. 2.—Olsen's 600,000-lb. Universal Testing Machine.

In order to test long tension members and long columns, provision had to be made for adjusting the cross-heads for the various lengths required. Provision also had to be made to guide the traveling cross-head to withstand eccentric loading or side stresses in column testing. To produce the required height and motion of the traveling cross-head four long screws were joined to the four main pulling screws by means of double-length nuts.

The lower cross-head is supported and operated by the motion of four bronze nuts rotating on these screws and placed one in each corner. A ball bearing takes the thrust on each nut thus greatly reducing the power required for their operation. These nuts are so connected to each other and the machine proper by spiral and bevel gearing that the rotation of the horizontal shaft operated by the clutch at R rotates a vertical shaft which transmits motion to the cross-head nuts at any position of the head. The nuts are all rotated simultaneously and the cross-head raised or lowered. The lower cross-head as run on the upper screws attains with ease a speed of 10 ins. per min. carrying the top cross-head with it.

The lower cross-head has extensions to each corner which fit in guides provided for the same in the guide columns W-W. These guide columns are four in number and are made in four sections. The bottom columns are 9 ft. long and the upper columns each 6 ft. long. These columns are bolted to the base cover, one in each corner, and are bound together at intervals by means of very wide panels as well as at the top by means of a series of four cover plates. By this method of securing the columns all four act to a certain degree as one column in resisting a side thrust on the cross-head.

Method of Absorbing Recoil.—To protect the knife edges of the lever system from the jar following the rupture of a specimen under a high load, two recoil cylinders are placed at S directly under the main frame of the machine, so that the recoil of the lever system, acting through the lower chord on the plunger of the cylinder, is gradually transmitted to the entire framework of the machine and dissipated, thus protecting the knife edges of the levers against the effect of such recoil. The operation of these cylinders, which are made of steel, is very simple. The jar at

rupture closes a valve in the cylinder and puts the oil in compression. In addition to these recoil cylinders the customary check rods and rubber recoil washers are provided.

Tools for Operation.—The tools for the various tests on a machine of this size and their adaptation to the machine differ in many respects from those used on smaller machines, and in the testing of smaller specimens.

For tension tests three sets of wedge grips are used to cover the large range of specimens for which the machine is adapted. The wedges are controlled by hand levers and operated simultaneously in each head. They are counterbalanced by springs at the back of the cross-head. This method of counterbalancing by means of springs is new and better than the old method by means of weights in the case of such heavy tools. A guard frame to protect the wedge tools and to keep large specimens from falling out of the machine on rupture is attached to each cross-head as shown. These frames are of cast steel with hinged cover-plates so that they may be thrown open when desired.

The compression tools consist mainly of a large 24-in. square plate to rest on the weighing table, a pair of hardened steel plates and a ball-socket plate with a 24-in. square surface designed to fit in the lower or compression head of the testing machine.

The transverse tools consist of a central block fitting in the cross-head with a 2½-in. steel cylinder bearing and two lower blocks fitting in a slot of the weighing table extensions. These blocks are new in design, having a swiveled socket motion crosswise and supporting rollers which permit free longitudinal motion at the ends of the beam under test. These rollers are held rigidly by clamps while the beam is adjusted on its bearings and are then released by a small hand lever.

Method of Control and Use of Machine.—For the complete operation and control of this machine four hand levers are placed at T, N, V, and M.

Lever T is the main speed-change lever which operates a double positive clutch connecting with gears on the countershaft. This lever may be placed in any one of three positions, to the left to operate the fast gears, to the right for the slow gears and in the center for throwing out all gear connections, thus allowing the countershaft to run idle.

Lever N is either for reversing the slow gearing at the end of the countershaft, or throwing it out of gear altogether.

Lever V controls the clutch on the horizontal shaft operating the traveling cross-heads.

Lever M operates the friction clutches on the countershaft, and serves to make all connections with the motor.

To run the lower head on the upper screws, the shifting lever T is placed in its center position, lever M is thrown to the left, thus operating the countershaft and the silent-chain drive connecting with the upper horizontal shaft. Here by means of lever V a clutch is thrown in or out to create the desired motion of the cross-head.

To operate the lower or pulling screws, lever M is thrown to the left for all speeds obtained directly through gears controlled by lever T, and to the right for all slow speeds obtained through gears at the end of the countershaft controlled by the lever N and operated through gears controlled by lever T. It is readily seen that by this method of control no two speeds can be put in play at the same time, since all the speeds must be operated through the friction-clutch lever M. The machine is thus proof against accidental breakage from this cause.

By operating the slow gears at T with the lever M to the left all speeds of cross-head from $\frac{1}{4}$ in. to 1 in. per min. may be obtained through the motor speed regulator. Likewise, if fast gears are connected at T, all speeds over $2\frac{1}{2}$ ins. per min., used for setting specimens in the head or taking up slack, are obtained. By operating the machine through the reverse back gearing speeds from 0.025 in. to 0.10 in. per min. are obtained.

The possiblity of reversing the machine through the back gears by means of lever V without reversing the motor is a special feature and one of great value in beam testing or for general compression tests.

The weighing of the load is similar to that on other smaller machines of this type and needs no explanation.

Calibration and Acceptance Tests.—To calibrate this machine a special set of cast steel standardizing levers were constructed, having a count of twenty and by their use 100,000 lbs. pressure was placed on the machine and all poises made of correct weight.

In reference to sensitiveness it may be stated that a 14-lb.

weight placed on the weighing table while under a load of 100,000 lbs. caused a marked movement of the scale beam.

The accuracy of the beam screw was tested for the entire length with the small poise and found to be correct.

To test the accuracy of the entire weighing system above 100,000 lbs., a specimen of special steel $2\frac{7}{8}$ ins. in diameter having an elastic limit in excess of 110,000 lbs. per sq. in., was held in the machine by special grips with spherical adjusting ends. An especially constructed duplex dial extensometer was placed on this bar, using a gauge-length of 24 ins., so that readings of extensions within the elastic limit could be read to 0.0001 in. for equal increments of load on the specimen. The results obtained from this test were entirely satisfactory, and proved the correctness of the machine up to its maximum capacity.

The acceptance tests of this machine were three in number in addition to the calibration already described and were made as follows in the Department of Civil Engineering of the University of Pennsylvania.

The tensile test to prove the strength of the machine, more especially as to gearing, cross-heads, gripping system and recoil was made on a 4-in. square steel bar. This bar was held at each of the various set places in the columns, and the maximum load of 600,000 lbs. applied. At the top position the bar was hack-sawed at the corners until it broke under a load of 592, 000 lbs.

The recoil cylinders responded immediately, and absorbed the force of the blow, thus protecting the knife edges of the lever system from injury.

The test of the weighing table extensions for beam tests was made on an "A" frame of 20 ft. span constructed by the University out of two twelve-inch pieces of yellow pine, and held together at the base by two 2-in. diameter rods. This frame was repeatedly loaded to 200,000 lbs. with no noticeable defect in the beam extensions. The deflections at the two extremities were obtained by means of a surveyor's level and for maximum loading amounted to 0.027 and 0.04 ft.

For the compression test a yellow pine stick 16 x 16 ins., 24 ft. 3 ins. long was used. The ends were sawed to a bevel giving a 12-in. inclination to the stick in its entire length. A load of 400,000 lbs. was applied which developed a side thrust of nearly 17,000 lbs.

near the upper ends of the guide columns. The four guide columns acted as a single column and no appreciable lateral set remained on removal of the load.

The acceptance tests, which were conducted by Professor H. C. Berry under the direction of Professor Edgar Marburg, proved entirely satisfactory and the machine was accepted by the University.

NEW FORMS OF PENDULUM TESTING MACHINES.

By Thorsten Y. Olsen.

The purpose of this paper is to describe two recent designs of tensile testing machines on the pendulum principle of weighing.

The first and smaller machine, Fig. 1, has several new and advantageous features not obtainable in small testing machines of

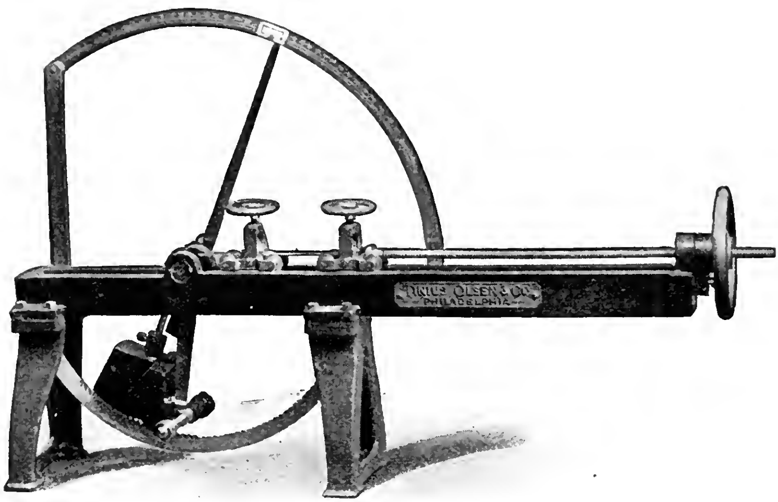


Fig. 1.—Olsen's Pendulum Textile Testing Machine.

other types. The straining mechanism is very simple and consists of a gripping carriage fastened to a screw operated by a hand wheel. The weighing mechanism involves a new principle in a weighing device to obtain a continuous scale reading of two different magnitudes.

The frame of the machine has two projections for the insertion of ball bearings in which swings the axle of the weighing pendulum. A steel tape connects the weighing carriage to the pendulum at a point about ninety degrees from its vertical axis.

At this point the steel tape extends and connects to a suspended weight which acts as a counterbalance to hold the pendulum, while at rest, in a position of approximately forty-five degrees to the vertical. A dog is fastened to an extension of the pendulum axle. This actuates a bar terminating in a slide moving over the graduated circular scale which is concentric with the axle of the pendulum. The purpose of this dog and slide is to provide a means of measuring the motion of the pendulum on the scale.

By creating a tension on the weighing carriage the pendulum is gradually lowered until it comes in contact with the counterbalance weight which up to this time has been slightly rising. The tension to this point of contact is approximately 12 lbs., which is graduated on the scale to read by 2-oz. marks. On application of further tension the pendulum moves, augmented by the addition of the suspended weight. This further motion of the pendulum represents a total tension of 200 lbs. and is graduated on the same scale to read from 12 to 200 lbs. by 1-lb. marks. The slide bar is counterbalanced and has a slight spring friction between back of scale and back of slide, so that the maximum tension is automatically recorded on the scale by the slide.

A circular rack is suspended below the frame and a pall is pivoted at the lower point of the pendulum in such a manner that it engages in the rack at the rupture of the specimen, thus keeping the pendulum from dropping and protecting it from injury. The pall is released from the rack and the pendulum returned to its position of rest by turning the small handle shown at the base.

By this method of weighing and automatically recording the maximum tension, the machine is adapted to all kinds of light commercial tests, from light yarn or thin paper to the heavier textile materials.

The second machine, Fig. 2, is intended primarily for wire tests, but may also be used for any other light test within its capacity, with proper gripping facilities.

This machine has two graduations on the scale, reading to maxima of 400 and 1,200 lbs. respectively. A weight on either side of the pendulum raises the capacity of the machine from 400 to 1,200 lbs. A set pointer is operated by a carrier on the axle of the pendulum and records the breaking load automatically as in the case of the machine first described. The pendulum is released

from the rack and returned to the initial position by means of the crank shown.

The weighing cross-head is connected to the pendulum by a knife edge and supported in its initial position by a pair of rollers.

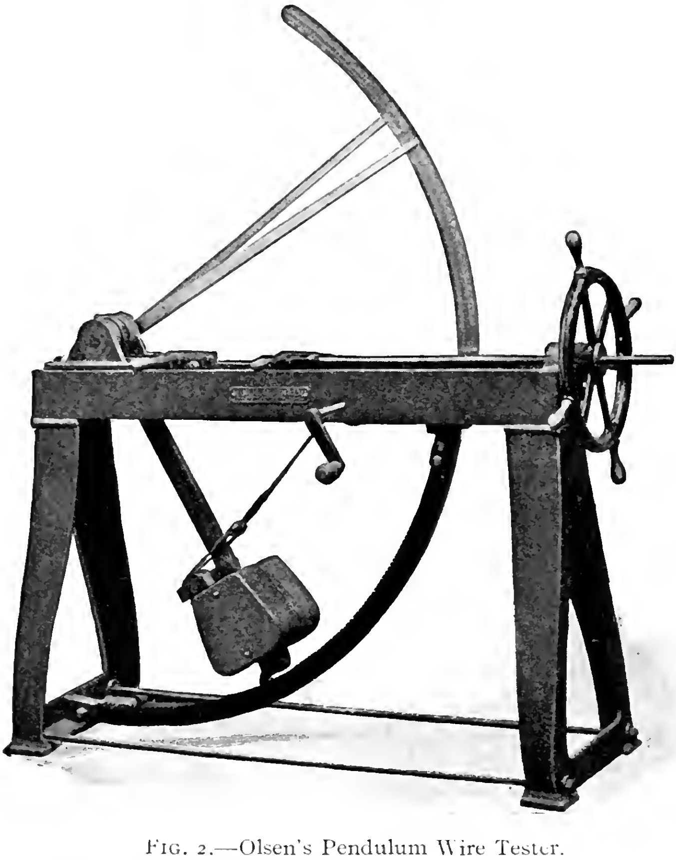


FIG. 2.—Olsen's Pendulum Wire Tester.

Very long wedges are provided with the machine and operated simultaneously in each head by means of hand levers.

The ease with which wire can be inserted in the heads, the automatic method of recording, and the rapid motion of the straining cross-head obtained by turning the hand wheel, insure quickness of operation, so that a large number of tests can be made in a short time with great accuracy.

In conclusion, it may be said that the first of these machines was designed to meet specifications which existing machines did not cover; and the second to meet the demand for an automatic quick-acting machine of moderate capacity and small cost.

THE USE OF THE EXTENSOMETER IN COMMERCIAL WORK.

BY T. D. LYNCH.

This paper has for its main object the presentation of a few representative stress-strain diagrams for steel and other metals in tension, showing in a general way their comparative elasticity phenomena. The curves selected are from the records of commercial work carried on in the laboratory of the Westinghouse Electric and Manufacturing Company at East Pittsburg, Pa. The apparatus used in making the tests consisted of a 100,000-lb. Olsen testing machine running at a speed of 0.0078 in. per min. and a Ewing extensometer reading directly to 0.00008 in. in $1\frac{1}{4}$ in., or 0.000064 in. per inch of length. This extensometer, while necessarily very delicate, is positive in its readings within relatively small limits of stretch. Tests made with such precision should be considered commercial only along the lines of investigation and design engineering.

While past experience makes it a comparatively simple matter to put into a machine enough material to insure its resisting any stress that can conceivably be put upon it, nevertheless, the more exact our knowledge of materials, the more safely and completely can we utilize their possibilities. In these days of close competition, ability to design high-efficiency apparatus of minimum cost and minimum weight has become a prime requisite, and to do his work effectively, the designer must have at hand accurate information which will enable him to select that material or grade of material best suited to meet the conditions which a careful preliminary analysis has shown to exist.

The curves accompanying this article represent actual tests on actual samples selected to show the relative characteristics that may be expected from various materials, rather than to give a limit of valuation or definite figures for the use of designing engineers. It should be clearly understood, however, that the only way to ascertain the true characteristics of a piece of material is to test a sample taken directly from the piece itself, so numerous and

far-reaching are the conditions that may arise during manufacture to change the properties of almost any piece.

It is an axiom that all strains in a design must be taken care of by a material worked below the elastic limit; and in this discussion let us not confuse the elastic limit with the yield point. The elastic limit is taken to mean that point at which the rate of stretch begins to increase, while the yield point is taken to mean that point at which the rate increases suddenly.

The yield points as given below were taken with the dividers, while the elastic limits were taken from the several curves. The yield point as determined by the dividers or by the drop of the beam is usually much higher than the true elastic limit and its determination is subject more or less to the personal equation, thus bringing in elements of chance or guesswork which, from considerations of safety, force the assumption of a large marginal factor. Data so acquired evidently cannot form a satisfactory basis for any exact analysis, either in the determination of sizes or in the proper selection of material for an important new design.

The art of testing materials is, however, fast becoming a science in itself and, with the large store of accurate information now available with regard to almost every material and the means of obtaining data about that not fully understood it is no longer necessary to guess how hard a material may be worked. It is becoming, in fact, more and more possible to say exactly how hard it shall be worked in order that the conditions of the problem may be best fulfilled. But new conditions are continually arising, new materials continually being developed to meet them, and this development work becomes fruitful only when the best means are provided for careful testing. Great care must be observed both in the selection of representative samples and in the actual making of tests if we are to know the true characteristics of a material under actual working conditions. Complete curves should be available on all important materials, and foremost among the properties to be determined from them is the elastic limit.

The question now properly arises, what elastic limit for a given material shall we use, and how closely may we approach this elastic limit and still be safe? This question has been asked of our laboratory so often that a large number of curves have been made covering various grades of steel and other materials, giving

the true curve up to and beyond the elastic limit; a few of these follow:

Fig. 1 shows a series of tests on a 2 x 2-in. soft open-hearth steel bar having an analysis of carbon, 0.16, phosphorus, 0.013, manganese, 0.48, and sulphur, 0.016. These three curves represent tests taken from the same bar, and show the average characteristics in both the normal manufactured state and after special treatment. Curve A represents the ordinary hot-rolled bar, and the regular test as commercially taken gave 59,000 lbs. tensile strength, 32,000 lbs. yield point, and $45\frac{1}{2}$ per cent. elongation in

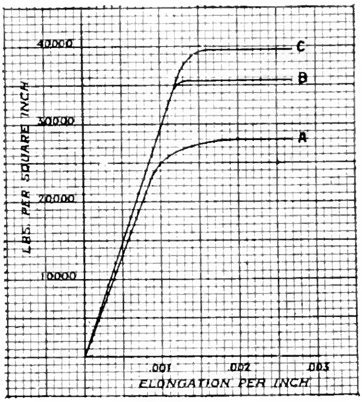


FIG. 1.

2 ins., while the curve shows 24,000 lbs. per sq. in. elastic limit, or an elastic limit of only 75 per cent. of the yield point.

Curve B represents the same bar oil tempered and annealed, with a commercial test of 59,000 lbs. tensile strength, 38,000 lbs. yield point, and $41\frac{1}{2}$ per cent. elongation in 2 ins., while the curve shows 35,000 lbs. per sq. in. elastic limit, or an elastic limit of 92 per cent. of the yield point.

Curve C represents the bar after it had been oil tempered, with a commercial test of 69,500 lbs. tensile strength, 44,000 lbs. yield point, and $38\frac{1}{2}$ per cent. elongation in 2 ins., while the curve shows 37,000 lbs. per sq. in. elastic limit, or an elastic limit of 84 per cent. of the yield point.

Fig. 2 shows a similar series of tests taken from a $1\frac{1}{2}$-in.

diameter axle-steel bar having an analysis of carbon, 0.37; phosphorus, 0.025; manganese, 0.37; sulphur, 0.022.

Curve A represents the commercial hot-rolled bar and the regular test gave 76,000 lbs. tensile strength, 49,000 lbs. yield point, and 33 per cent. elongation in 2 ins., while the curve shows 46,500 lbs. per sq. in. elastic limit, or an elastic limit of 95 per cent. of the yield point.

Curve B represents the same bar after it had been oil tempered

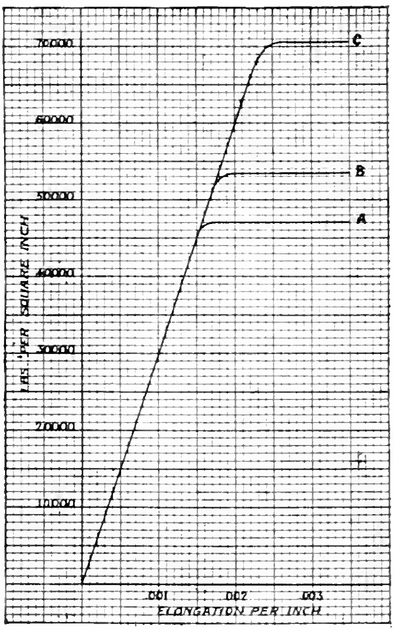


FIG. 2.

and annealed, and gave 86,500 lbs. tensile strength, 55,000 lbs. yield point, and 29 per cent. elongation in 2 ins., while the curve shows 52,500 lbs. per sq. in. elastic limit, or an elastic limit of 95 per cent. of the yield point.

Curve C represents the bar after it had been oil tempered, with a commercial test of 106,500 lbs. tensile strength, 71,500 lbs. yield point, and 21½ per cent. elongation in 2 ins., while the curve shows 67,000 lbs. per sq. in. elastic limit, or an elastic limit of 93.7 per cent. of the yield point.

Fig. 3 shows a third similar series of tests taken from a disc forged from a 9½-in. square billet to a cross section of 6 ins. square, from which a section 7 ins. long was cut. This was re-heated upset, and rounded into a disc 5¼ ins. long by 7¾ ins. in diameter, thus working the material in all directions. The analysis was: carbon, 0.48; phosphorus, 0.014; manganese, 0.59; sulphur, 0.013

Curve A represents the disc after it had been thoroughly annealed and the commercial test gave 79,000 lbs. tensile strength, 38,000 lbs. yield point, and 22½ per cent. elongation in 2 ins., while

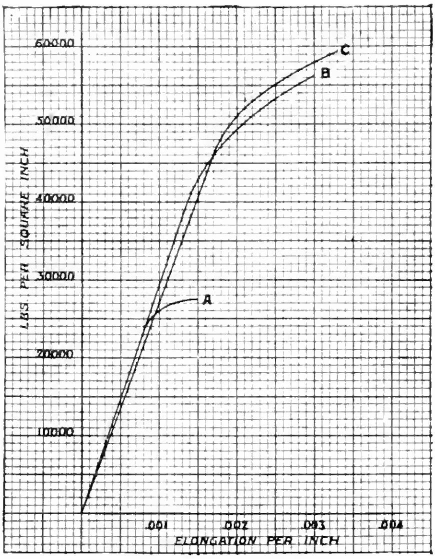


FIG. 3.

the curve shows 25,000 lbs. per sq. in. elastic limit, or an elastic limit of 66 per cent. of the yield point.

Curve B represents the same disc oil tempered and annealed with a commercial test of 98,500 lbs. tensile strength, 65,000 lbs. yield point, and 23 per cent. elongation in 2 ins., while the curve shows 41,000 lbs. per sq. in. elastic limit, or an elastic limit of 63 per cent. of the yield point.

Curve C represents the disc oil tempered with a commercial test of 130,000 lbs. tensile strength, 90,000 lbs. yield point, and 22½ per cent. elongation in 2 ins., while the curve shows 47,000 lbs.

per sq. in. elastic limit, or an elastic limit of only 52 per cent. of the yield point.

Fig. 4 shows two tests taken from a large steel disc cast solid, 38 ins. in diameter, weighing 15,000 lbs. after the riser had been cut off. This casting was thoroughly annealed and the tests were taken with hollow drill at half the radial distance, one at each end.

Curve A shows the top test. Analysis: carbon, 0.36; phosphorus, 0.044; manganese, 0.57; sulphur, 0.035. Commercial test: 68,500 lbs. tensile strength, 35,000 lbs. yield point, and 21 per cent. elongation in 2 ins., while the curve shows 23,000 lbs. per sq. in. elastic limit, or an elastic limit of 65.7 per cent. of the yield point.

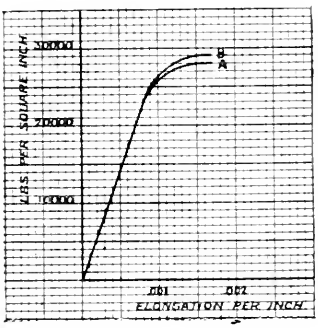


FIG. 4.

Curve B shows the bottom test. Analysis: carbon, 0.34; phosphorus, 0.047; manganese, 0.58; sulphur, 0.033. Commercial test: 70,500 lbs. tensile strength, 41,000 lbs. yield point, and 27.5 per cent. elongation in 2 ins., while the curve shows 24,000 lbs. per sq. in. elastic limit, or an elastic limit of 58½ per cent. of the yield point.

Fig. 5 shows a series of tests on copper in the three states or conditions very familiar to all users of copper.

Curve A represents a test from pig copper soft annealed, having a commercial test of 23,000 lbs. tensile strength, 36.5 per cent. elongation in 2 ins., and 2,500 lbs. per sq. in. elastic limit as shown on the curve.

Curve B represents a test from cast pig copper without treatment, having a commercial test of 33,000 lbs. tensile strength

39.5 per cent. elongation in 2 ins., and 12,000 lbs. per sq. in. elastic limit, as shown on the curve.

Curve C represents a test from a $\frac{3}{4}$-in. copper plate cold rolled, having a commercial test of 50,260 lbs. tensile strength, 19 per cent. elongation in 2 ins., and 25,000 lbs. per sq. in. elastic limit, as shown on the curve.

Fig. 6 is a summary of the curves already shown in Figs. 1 to 5 inclusive, with the addition of one curve each for cast iron, wrought iron, unannealed steel casting, nickel steel, chrome tungsten steel and chrome vanadium spring steel.

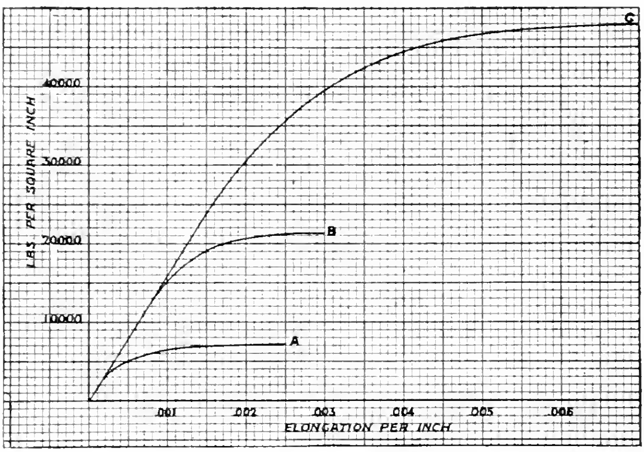


FIG. 5.

Curves 1–A, 1–B and 1–C represent soft steel as shown in Fig. 1.

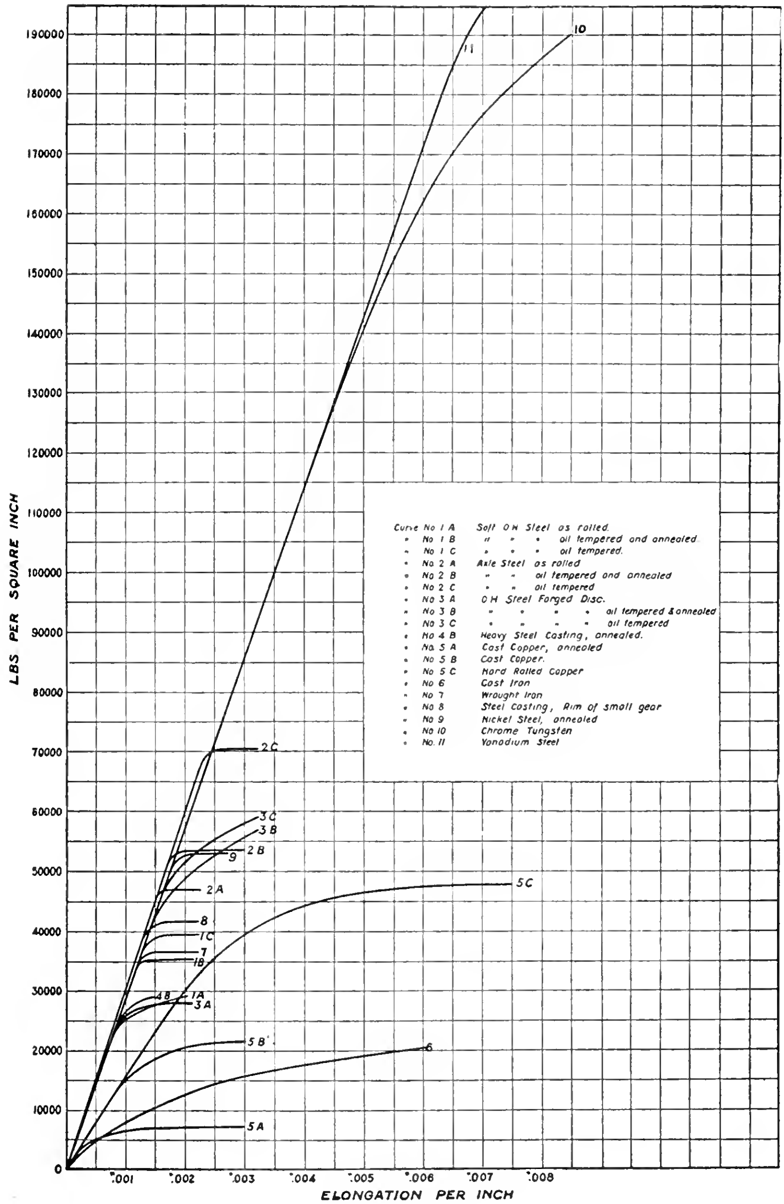
Curves 2–A, 2–B and 2–C represent axle steel as shown in Fig. 2.

Curves 3–A, 3–B and 3–C represent a forged disc as shown in Fig. 3.

Curve 4–B represents a bottom test cut from a heavy steel casting as shown in Fig. 4.

Curves 5–A, 5–B and 5–C represent copper, as shown in Fig. 5, each curve representing their several treatments as described above.

Curve 6 represents soft cast iron of about 20,000 lbs. per sq. in. tensile strength.


190000
180000
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10000
0
LBS PER SQUARE INCH
.001
.002
.003
.004
.005
.006
.007
.008
ELONGATION PER INCH
10
11
2C
3C
3B
2B
9
2A
5C
8
1C
7
1B
4B
1A
3A
5B
6
5A
Curve No 1 A Soft O H Steel as rolled.
" No 1 B " " " oil tempered and annealed
" No 1 C " " " oil tempered.
" No 2 A Axle Steel as rolled
" No 2 B " " oil tempered and annealed
" No 2 C " " oil tempered
" No 3 A O H Steel Forged Disc.
" No 3 B " " " " oil tempered & annealed
" No 3 C " " " " oil tempered
" No 4 B Heavy Steel Casting, annealed.
" No 5 A Cast Copper, annealed
" No 5 B Cast Copper.
" No 5 C Hard Rolled Copper
" No 6 Cast Iron
" No 7 Wrought Iron
" No 8 Steel Casting, Rim of small gear
" No 9 Nickel Steel, annealed
" No 10 Chrome Tungsten
" No. 11 Vanadium Steel

FIG. 6.

Curve 7, wrought iron.

Curve 8, unannealed steel casting cut from the rim of a gear.

Curve 9, nickel steel, annealed.

Curve 10, chrome tungsten steel.

Curve 11, chrome vanadium spring steel.

The slopes of these curves show the characteristics of the different materials. Special attention is called to the fact that the steels of all grades and tempers, together with wrought iron, have approximately the same modulus of elasticity. Copper, soft annealed, cast and hard rolled has a lower modulus, while cast iron gives us a still lower modulus with a curve different in shape from any of the other materials shown in the series.

Fig. 7 illustrates peculiar phenomena of steel under repeated stress and rest and in a measure at least shows the effect of cold rolling where time has elapsed between the different passes.

Series A represents a hard brittle steel, analysis: carbon, 0.57; phosphorus, 0.107; manganese, 1.10; sulphur, 0.066.

Series B represents a tough medium grade of steel low in phosphorus, manganese, and sulphur. Both series were tested in the same manner.

All curves, 1 to 10 inclusive, were made from the same test sample, one in each series. One day intervened between the time of making 1 and 2, two days between 2 and 3; one day each between 3 and 4, 4 and 5, 5 and 6, 6 and 7, and 7 and 8; two days between 8 and 9; and one day between 9 and 10.

The original areas were used in computing the stress per square inch in both series, no account being taken of the slightly reduced area after each load was applied. There is a marked increase in the elastic limit, and a decrease in the rate of increase, from the first to the tenth application of stress. It is also very marked how similar the ductile and brittle steels are in the relative hardening effect shown in the two series.

Fig. 8 illustrates the resilience of steel below the elastic limit and what we may expect if we exceed the elastic limit in a shrink or press fit.

Curve A–B–C–D–E was made on open-hearth steel, analysis: carbon, 0.28; phosphorus, 0.066; manganese, 0.567; and sulphur, 0.057. 31,500 lbs. per sq. in. was applied with the elongation of 0.001 in. (point A). This tension was held for thirty minutes

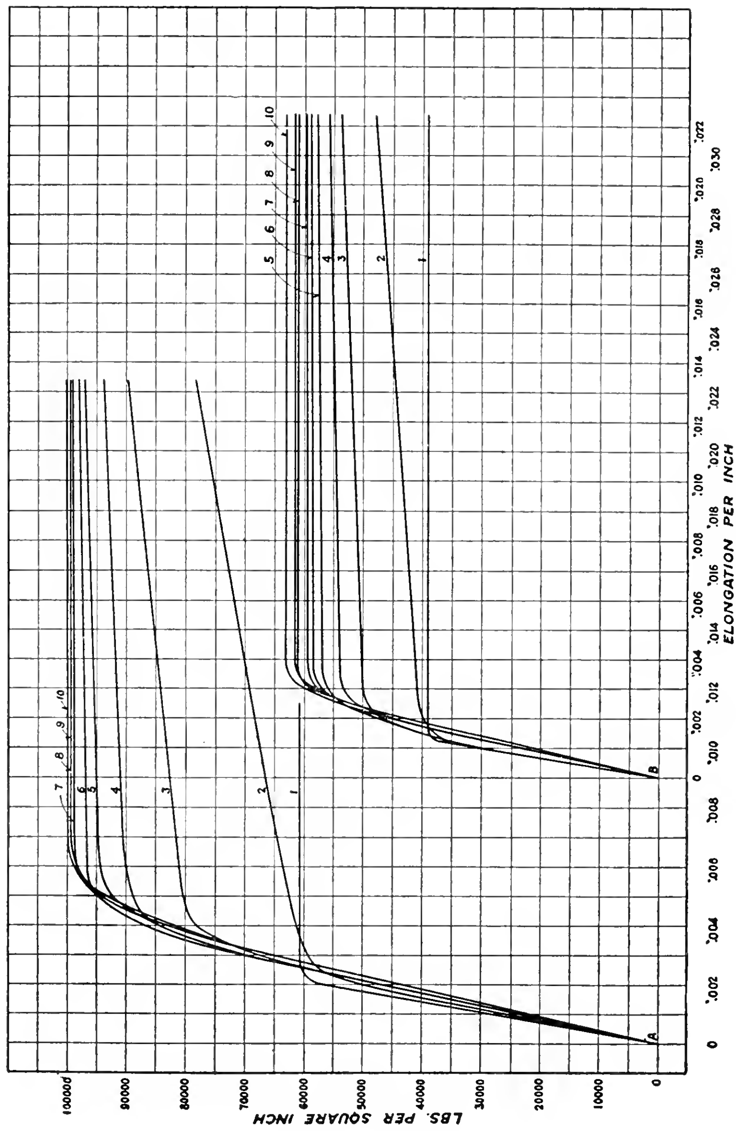
ELONGATION PER INCH
LBS. PER SQUARE INCH
A
B
0
.002
.004
.006
.008
.010
.012
.014
.016
.018
.020
.022
.024
.026
.028
.030
10000
20000
30000
40000
50000
60000
70000
80000
90000
100000

FIG. 7.

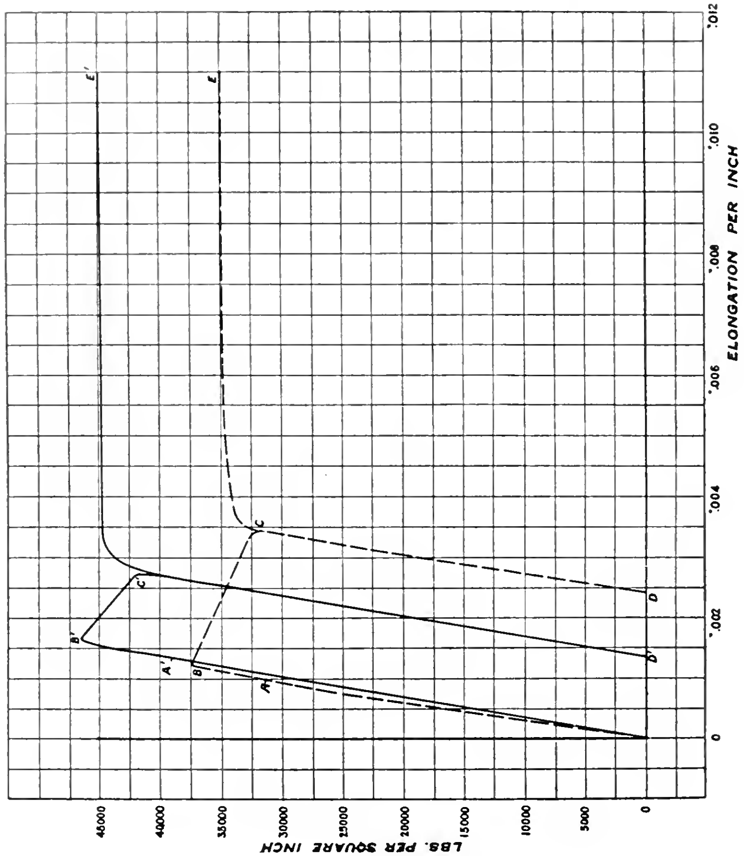

FIG. 8.

and released after which the sample showed no stretch. A second stress of 37,300 lbs. per sq. in. was applied, the material having just passed the elastic limit (point B). The stress was kept on and in two minutes the sample had stretched to point C, or 0.0022 in., without any additional tension being applied; on the contrary, the load dropped to 32,000 lbs. per sq. in. The specimen was allowed to stand in the machine thirty minutes longer with no additional stretch, after which the stress was removed, giving curve C–D parallel to the original curve O–A. Stress was again applied giving an elongation as shown on curve D–C–E. A second test was made on axle steel giving curve A′–B′–C′–D′–E′.

The characteristics were similar with a higher elastic limit, showing the stretch and corresponding loss of grip or holding power of a press or shrink fit when the material is worked beyond the elastic limit; also, that steel assumes a new holding value after the first stretching of the material has ceased.

Engineers differ in their opinions with reference to the safe stress at which various material may be used and it is the intent of this paper to suggest no rules to follow but simply to leave with you a record of these few tests.

In closing the writer wishes to acknowledge his indebtedness to R. T. Elliott, of the Physical Laboratory of the Westinghouse Electric and Manufacturing Company, for special care in making the tests reported in this paper.

DISCUSSION.

Mr. Shuman.

MR. J. J. SHUMAN.—I feel that the Society owes thanks to Mr. Lynch for his careful paper. I for one wish to state that I have gained information on the subject of the elastic limit. I suppose it is necessary to use a tool of the type employed by Mr. Lynch in order to bring out the fact that the true elastic limit of steel and other metals is considerably lower than the elastic limit as usually observed. Now if steel of 59,000 pounds tensile strength has only 27,000 or 28,000 pounds elastic limit, it would seem that it is not right that any specification should read, "The elastic limit shall be at least 50 per cent. of the ultimate strength;" and I am glad to note that our Society has already begun to discourage the use of that clause.

It occurs to me to ask what figures we ought to take as the true values which engineers might use in calculating the strength of their materials. Mr. Lynch has shown us that the real elastic limit is perhaps 20 per cent. lower than the figure that has generally been accepted; yet in his definition he states that the yield point is the point at which the stretch becomes suddenly pronounced. Now which is the dangerous point in a piece of steel, the point indicated by Mr. Lynch's chart, or the point at which the steel suddenly yields? I hope Mr. Lynch can answer this question, although I realize that he has stated that he has merely presented the facts as they have come to him, with no intention of answering questions of this kind.

Mr. Capp.

MR. J. A. CAPP.—There is one point that Mr. Lynch has shown in his paper that is often lost on many designing engineers. They are carried away with the idea that if they use a stronger steel with a higher elastic limit, they can use smaller sections, and forget that in doing so they are using material of the same modulus of elasticity, and that therefore they are greatly increasing the deflection. If you follow the curve No. 6, the straight line portion of the curve which measures the modulus of elasticity is a straight line, and the several grades of steels branch off from that line at higher or lower points. We run into that occasionally among our designing engineers who want to make smaller shafts to carry a definite load, whereas we have to design our shafts for stiffness rather than strength.

AN AUTOGRAPHIC RECORDER FOR RAPID TENSION TESTING.

By H. F. Moore.

At the Ninth Annual Meeting of the Society, in the course of a discussion on "Uniform Speed in Testing," (Volume VI., pp. 120–125), the fact was brought out that there was in use no autographic attachment for testing machines suitable for rapid commercial work. One speaker said that such an apparatus should draw a diagram from which could be obtained the modulus of elasticity, the elongation, the elastic limit, the yield point, the ultimate strength, and the work of rupture. It may be questioned whether the determination of the modulus of elasticity or of the elastic limit as distinguished from the yield point is necessary to a satisfactory report of a commercial test of iron or steel. The modulus of elasticity varies between narrow limits for all grades of iron and steel, and so far as the writer knows its value has never been proposed as a determining factor in any specifications for those metals. The elastic limit or "true" elastic limit (perhaps better named the limit of proportionality) is that intensity of stress at which Hooke's Law ceases to be exact; this stress is never found exactly even in the most refined laboratory tests. The more delicately the deformation of the specimen is measured the lower is the value obtained for the limit of proportionality. As an illustration of this may be quoted data recently obtained in the Materials Testing Laboratory of the University of Illinois. In a test of soft steel two extensometers were used on the same test piece, with the centers of their gauge lengths at the same cross-section of the specimen. One extensometer was a micrometer type instrument reading to ten-thousandths of an inch over a gauge length of two inches; the other a Ewing extensometer reading to fifty-thousandths of an inch over a gauge length of eight inchs. By the micrometer type instrument, the limit of proportionality was located at a stress of 42,100 lbs. per sq. in., while by the Ewing instrument the limit of proportionality was located at a stress of 39,650 lbs. per sq. in. As a matter of fact not a few

engineers and physicists question the existence of a rigid limit of proportionality of stress to deformation for any materials in commercial use.*

The objection to the common forms of autographic attachment supplied with American testing machines is their slowness of operation. More time is consumed in attaching the apparatus to the specimen than is required for a complete test by ordinary commercial methods carefully used. The objections to the ordinary electric devices for automatically keeping the weighing beam balanced are their unreliability and the jerkiness of their action.

An autographic apparatus which in its operation is entirely free from handling by the operator, which draws a diagram from which may be determined the yield point, the ultimate strength, and with a fair degree of accuracy the elongation and the work of rupture, and which can be used at the speeds of commercial testing, has been in use during the past year in the Materials Testing Laboratory of the University of Illinois.† A steam engine indicator is used to record both stress and elongation, the spring of the indicator being attached to the beam of the testing machine, while the cord of the indicator is attached to the pulling head of the testing machine. Of course if the motion of the head is taken as the extension of the specimen, all the slip of the grips is measured as a part of the stretch of the specimen, and this slip would render utterly unreliable the measurements of the very small elongations within the yield point. Beyond the yield point, however, the stretch of the specimen is so greatly increased that no very serious error is introduced by including the slip of the grips. If the standard threaded specimen is used, the slip is very small indeed. The yield point is plainly shown, both with threaded specimens and with specimens held by wedge grips. If the grips slip before the yield point, the card drawn shows the curve resuming its original direction when the grips catch again, and the yield point is not masked.

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* Popplewell, "Experimental Engineering, Materials of Construction," p. 10; Unwin, "Testing of Materials," p. 13.

† Since the presentation of this paper, the writer has learned from a letter from Mr. J. H. Wicksteed that at the well-known English works of Joshua Buckton & Co. a device very similar to the one described above has been in use for some time. So far as the writer knows this English device has not been described in print, but it was placed on exhibition in London at about the same time this paper was presented.

The apparatus is shown in more detail in Fig. 1. To the beam of a 100,000-lb. Riehlé testing machine is attached a steam engine indicator I, the rod R bearing at one end A on a center at the rear end of the testing machine beam, and at the other end bearing against the center of the piston of the indicator. The poise of the testing machine is run out until the pressure on the rod R, compressing the indicator spring, forces the pencil nearly to the top of its stroke. The poise is then locked in that position. As load is applied to the specimen S, instead of running out the poise to preserve equilibrium the beam is allowed to rise. This causes the rear end A of the beam to drop,

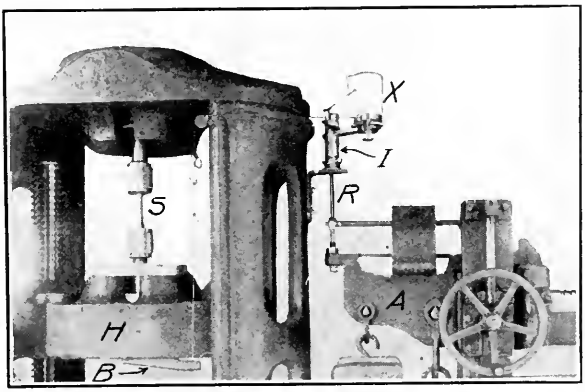


FIG. 1.

and consequently releases the load on the spring which elongates until equilibrium is again established, the elongation of the spring and the consequent downward motion of the pencil being proportional to the load applied. The amount of rotation of the indicator drum X shows the amount of motion of the head and, disregarding the slip of the grips, the stretch of the specimen. In an actual test, the specimen being in place, the machine may be started and the indicator cord then wrapped round the arm B and caught. The load-elongation diagram is then drawn on the indicator card without further attention on the part of the operator, who can be measuring the next specimen while the test is proceeding.

The scale of stresses may be determined at any time by moving the testing machine poise through any given distance

along the beam, and noting the corresponding motion of the indicator pencil.

Some idea of the accuracy of the device as used at the University of Illinois is given by the following representative data of tests:

Material, Soft Steel Rod, All Specimens from One Piece.

	Autographic Device.		"Drop of Beam" Method.	
	Test 1.	Test 2.	Test 3.	Test 4.
Load at yield point (lbs.)	7,790	7,070	8,260	8,030
Unit-stress at yield point (lbs. per sq. in.)	38,000	38,900	40,300	39,300
Load, ultimate (lbs.)	12,400	12,400	12,670	12,690
Unit-stress, ultimate (lbs. per sq. in.)	60,500	60,500	61,700	61,900

In the above tests the "drop of the beam" gave the yield point very closely since the material was soft steel with a very clearly defined yield point, and on the "drop of the beam" tests the testing machine was run at a speed of pulling head of 0.1 in. per min. In the tests with the autographic apparatus the pulling speed was 1 in. per min. Other tests of the same material with the autographic apparatus, using a pulling speed of 0.1 in. per min. gave the same yield point.

In a somewhat extensive series of tests of reinforcing rods for concrete this device was tried in comparison with the "drop of the beam" method, and was found to be equally rapid in operation. In a recent test the writer broke six boiler-plate specimens in twenty-four minutes, measuring and marking the specimens and making the necessary notes within that time. The test was made without the aid of an assistant.

Sample cards are submitted (Figs. 2 and 3). The apparatus as arranged drew cards from right to left. The sudden jump upward after rupture is caused by the recoil of the tension head and columns of the machine. This jump in the curve is not a part of the load-elongation diagram, and introduces no error in that diagram. The jump might be obviated by attaching the indicator to the framework of the machine instead of to the tension columns.

The steam engine indicator was used in the device because it was available and because it had an accurate spring and pencil motion. The usual size of indicator gives cards which are smaller

than desirable, and the card must be placed on the drum for every specimen tested. An apparatus so made as to give a larger card, and in which the paper could be fed from a long roll, would be a distinct improvement over this preliminary device.

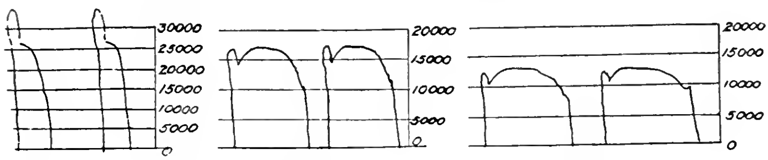


FIG. 2.—Cards from threaded specimens, reduced section 2½ ins. long.

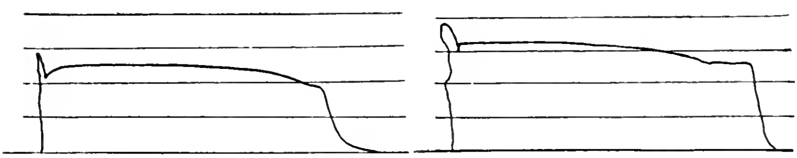


FIG. 3.—Cards from flat specimens 4 ins. between grips, held by wedge grips.

The writer believes that the application of some autographic device like this to testing machines used for commercial testing would be advantageous for the following reasons:

With the report of each test would be furnished a load-deformation diagram.

The personal equation in handling the testing-machine beam would be eliminated.

The speed of testing would not be reduced.

The device could be applied to the ordinary type of American testing machine, and could be calibrated at any time.

UNIFORMITY IN MAGNETIC TESTING AND IN THE SPECIFICATION OF MAGNETIC PROPERTIES.

By Chas. W. Burrows.

The present paper deals with the determination of the magnetic data of straight bars of iron and steel. Apparatus for this purpose may be primarily for commerical testing or primarily for the use of a standardizing laboratory where the highest accuracy is demanded.

Commercial Permeameters.

A good commercial permeameter should have few or no delicately adjusted parts to get out of order. It should be so simple of manipulation that a skilled experimenter is not required. It must yield results rapidly and consistently. It should be adapted to a specimen which may be prepared readily and cheaply. Any corrections that are to be made should not be too complicated and should be susceptible of verification.

One of the best known of the commercial permeameters is that invented by Koepsel in 1890. It is shown diagrammatically in Fig. 1. The test specimen is placed within the magnetizing coil C and firmly clamped by means of thumb screws to the heavy quadrantal yokes. These yokes embrace the moving coil to which is attached a pointer moving over a scale. A fixed electric current flowing through the movable coil causes a deflection proportional to the induction in the specimen. By proper adjustment of the constants, the induction and magnetizing force may be read directly from the scale of the instrument and an ammeter in series with the magnetizing current. The instrument is of the moving coil galvanometer or voltmeter type, except that the current is fixed and the field strength varied.

Esterline, in 1897, modified this apparatus by replacing the moving coil by a revolving armature, driven by a motor. The E. M. F. generated is proportional to the induction in the specimen, and may be measured by an ordinary voltmeter.

Carpentier (1903) places a moving magnetic system over the air gap and by means of a torsion fiber balances the resultant torque.

These three instruments might be placed at the head of a long list of apparatus intended for rapid magnetic work. The first two, in addition to the qualities already enumerated as desirable, are direct reading. For accurate commercial work they are all subject to certain corrections which may be expressed in the form of a

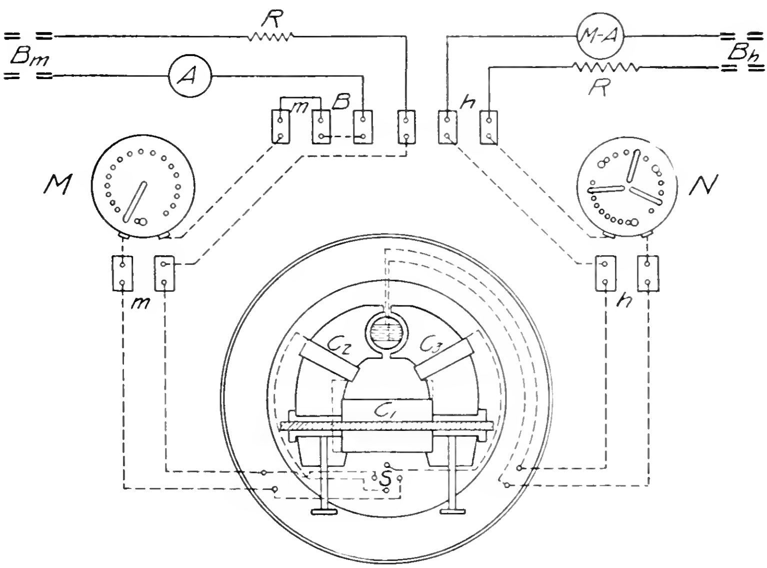


FIG. 1.—Koepsel Permeameter. C_1, main magnetizing coil surrounding the specimen. C_2 and C_3, auxiliary coils to eliminate the effect of C_1 alone upon the movable coil. On the left are the battery, resistances and ammeter in series with the magnetizing coils, while on the right are those of the moving coil.

correction curve. The correction is a function of the induction but depends somewhat on the quality and form of the specimen. Consequently it is desirable to calibrate a commercial instrument with a standard rod of approximately the shape and quality of the specimens to be measured. If a wide range is desired there should be more than one calibration.

STANDARDIZING APPARATUS.

In the second class of magnetic apparatus, that intended for standardizing rods to be used in the calibration of commercial apparatus, simplicity and rapidity of operation are less important, but higher accuracy is required. In these absolute determinations there must be no correction factors except those susceptible of calculation or elimination.

In Rowland's ring method (1873) there are no end effects to be corrected for. In the magnetometric method using an ellipsoid the end effects may be calculated. Cylindrical specimens, however, are not adapted to these methods. The only methods suitable for straight bars are the Ewing double yoke and double length method described in 1896, the Picou method described in 1902, and the method employed at the National Bureau of Standards, which has not yet been published.

In Ewing's double yoke and double length method two rods and two small yokes form the sides of a rectangle. Complete sets of data are obtained for each of two different lengths of the same rods. From these two sets of data it is possible to eliminate the effects of the yokes and thus obtain the magnetic data for the rods alone.

This method assumes uniformity in induction along the specimen and also that the two rods are magnetically equal. These two assumptions are not fully met and it is impossible to estimate the magnitude of the error. Furthermore it requires two specimens and a double set of data, thus giving greater chance for experimental error.

The yokes in the Picou apparatus consist of two U-shaped pieces between the ends of whose legs the test specimen is clamped. Magnetizing coils are wound around the yokes and also around the specimen. The magnetizing coils which surround the yokes are first connected so as to magnetize the two yokes in series, as shown by the dotted lines and arrows. In this arrangement these coils furnish enough magnetomotive force to overcome the reluctance of the yokes and joints and twice the thickness of the rod. No flux, however, passes longitudinally through the specimen. The current is then reversed about one of the yokes thus putting the yokes magnetically in parallel with each other and in series with the test rod, as shown by the dot-and-dash lines and arrows. A

sufficient current is then passed through the coil surrounding the specimen to overcome the extra magnetic reluctance added by the specimen and to restore the flux in the yokes to the former value. These adjustments are made by means of an adjustable transformer T and suitable test coils in connection with a galvanometer. After these adjustments the flux in the rod is measured ballistically in the usual manner.

In this method the compensation for reluctance of the yokes is not complete, due to the fact that it is not distributed in proportion to the reluctance of the circuit. It is impossible to estimate the error introduced by this. Furthermore the adjustment depends upon the speed of reversal of the commutator. It is impossible to set the apparatus to predetermined values of H and B. The method has the great advantage, however, of being applicable to a single rod.

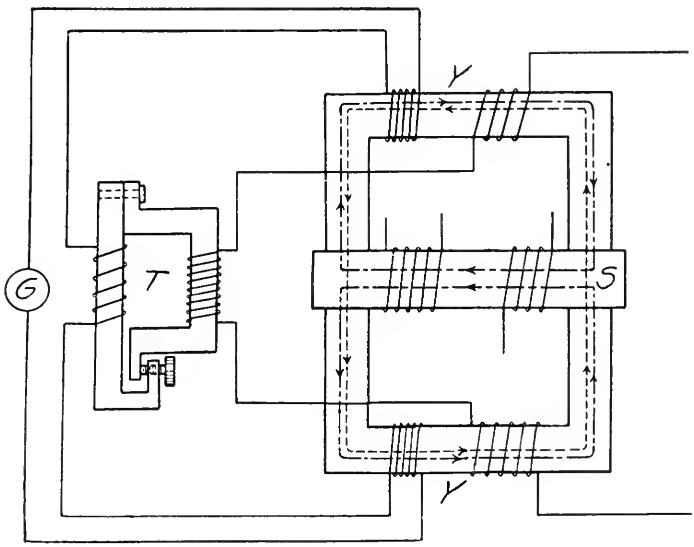


FIG. 2.—Showing the essential features of the Picou permeameter. Y-Y, yokes. S, test specimen. T, variable transformer. G, galvanometer. Switches and commutator are not shown.

Consider for a moment a long uniform rod surrounded by a uniformly wound solenoid, carrying current. The rod is uniformly

magnetized throughout its length. The condition for uniform flux is that the magnetizing force divided by the reluctance, or $\frac{H}{R}$, shall be constant. If, however, there is a portion of the rod which has a greater permeability or a greater section than the rest of the rod, the flux in this region will be greater because $\frac{H}{R}$ is greater. The uniformity of flux may be restored by removing some of the magnetizing turns encircling this region. If at another point there is a region of greater reluctance, there will again be a nonuniformity of flux unless the magnetizing force over this region is increased. Thus a uniformity of flux may be secured in spite of inhomogeneities, provided the magnetizing force is so distributed that throughout the circuit $\frac{H}{R}$ = constant.

Such inhomogeneities as these are found in the ordinary bar and yoke methods of magnetic measurement. The yoke usually has a smaller reluctance than the bar while the joint connecting the two has a higher reluctance. Ordinarily there are no magnetizing turns over the yokes so that the magnetizing force, the reluctance, and usually the flux, have nonuniformities at the yokes. Properly distributed compensating turns may restore uniformity of flux. This is the basis of the method used in the magnetic measurements at the National Bureau of Standards.

This method was proposed by Professor E. B. Rosa, who is in charge of the electrical and magnetic work of the Bureau, and in its development I have had his coöperation and assistance.

Bureau of Standards Method.

From certain more or less obvious considerations it appears desirable to have the specimen long and slender, the yokes of the same cross section as the test specimen, well surfaced joints, uniformly wound coils fitting closely over both yoke and specimen and an additional coil over the joint.

Owing to practical considerations, however, certain modifications must be made in the above ideal conditions. The magnetic circuit may be composed advantageously of two short yokes and two bars, one of which is the test specimen. The magnetomotive force is applied in three sections, one over the test specimen, one over the second or auxiliary rod, and the third distributed over the

four joints. These are connected in series and the individual coils shunted so as to vary the relative magnetomotive forces developed. By means of suitable test coils distributed over various parts of the circuit, it is possible to adjust the three sections of magnetomotive force so that there is approximately uniform flux throughout the magnetic circuit. When this adjustment is made the corresponding values of B and H may be obtained from a test coil wound over the middle of the test specimen and from the concentration of magnetizing turns immediately over this test coil. In practice three test coils are used. One of these is placed over the middle of the test specimen; a second is divided in two sections and distributed over the ends of the test specimen. The third is over the middle of the auxiliary rod. If the auxiliary rod is of the same material as the test specimen, the time of a test is greatly reduced.

The induction is measured by connecting the test coil surrounding the specimen in opposition to the secondary of a variable mutual inductance, and adjusting the current in the primary of the mutual inductance until no deflection of the galvanometer is noted on reversal of the two primary currents.

The variable mutual inductance is of wide range and is adjusted for each specimen to such a value that the induction in the test specimen is given by the relation,—

$$B = 10{,}000\ I,$$

where I is the primary current.

In the adjustment of uniform flux and of the current in the available inductance the ballistic throws are in each case due to the difference of two E. M. F.'s which is eventually reduced to zero. Consequently one may use his galvanometer at maximum sensibility and is not concerned with its constancy or calibration.

All current measurements are made with a potentiometer and resistance standards. The resistance standard used in the measurement of the magnetizing current is of such a value that the potentiometer reading differs from the magnetizing force by a power of ten. Thus the values of both magnetizing force and induction are read directly from the potentiometer with no further calculation than the shifting of a decimal point. By means of this apparatus it is possible to set to any predetermined value of induction or of magnetic force. This is a very decided advantage. In

this third method it is possible to set limits to the possible error due to imperfect adjustment of the compensating magnetomotive forces, by comparing results with overcompensation with those with undercompensation. The error due to imperfect compensation is very small and may be reduced in proportion to the patience of the experimenter.

In spite of the sources of error mentioned, these three methods give surprisingly consistent results.

METHOD OF PROCEDURE.

The induction in iron depends on the actual value of the magnetizing force, the previous magnetic condition and also the manner in which the magnetizing force is applied or altered. If consistent results are to be obtained by different methods the iron must receive the same magnetic treatment in each case. This is especially important when a standard permeameter rod measured by one of the absolute methods is to be used in the calibration of a commercial instrument.

Standard permeameter rods are measured at the National Bureau of Standards under the following conditions, which were selected after a large amount of experimental investigation. The specimen is clamped in the permeameter and carefully demagnetized by making one or two reversals per second of a magnetizing current which decreases slowly from an initial point well above the knee of the curve to a final value somewhat lower than the lowest value to be used in the test. After thoroughly demagnetizing in this way the smallest magnetizing force to be studied is applied and reversed may times till the iron is in a cyclic state. During this process the compensation for uniformity of flux is made. The number of reversals necessary to bring about a cyclic condition varies from a half dozen in the region of high inductions, to several hundred in the steepest part of the induction curve. In general a soft steel is slower in reaching a cyclic state than a hard one. The induction resulting after this process is the quantity to be measured. For the highest accuracy it is necessary that the earth's field does not act longitudinally on the specimen, otherwise the lower inductions will be in error. It is further desirable that the apparatus be protected from mechanical vibration. Such vibrations cause a higher apparent induction than normal. In commercial instru-

ments which measure the field due to the magnetized specimen care must be taken to eliminate the effect of the earth's field and any strong magnets, particularly in measuring instruments, which may be near by.

For a hysteresis curve the iron is caused to pass through the cycle several times by reversing the maximum magnetizing force. Then the current is reduced at one step to a smaller value and the corresponding induction determined. After this first point on the hysteresis loop is obtained the iron is carried through the original cycle several times as before and a new point determined. In this way as many points as desired are obtained. Negative values of H are obtained by reducing and reversing the magnetizing force at one operation. By this procedure each point is an independent determination and is independent of any appreciable effect due to magnetic viscosity or to the number of steps.

Since a correction curve must be obtained for every hysteresis loop having a different maximum induction it would seem very desirable to fix upon some one maximum value for a standard hysteresis loop. The Hysteresis Kommission of the German Electro-Technical Society has suggested a maximum induction of 10,000 as a suitable standard value, and this is the value which we are using at the Bureau.

THE TEST PIECE.

From the magnetic standpoint the test piece should be long and slender and so surfaced as to make a closely fitting joint with the yokes. Most apparatus call for a turned specimen. This means that great care must be exercised in having the test specimen and permeameter hole exactly to size and requires special bushings if there is to be any variation from size. It is quite difficult to get bushings and specimens to fit perfectly. Any variation in the contact means a variation in the distribution of leakage at this point and a consequent variation in the correction factor. It is better to use a specimen of rectangular section as it is much easier to get a good contact between two flat surfaces. Furthermore slight variations in size do not impair the magnetic contact, and bushings are not necessary. Mechanically a long slender rod is difficult to prepare, and a compromise might be made on a rod 35 cm. long and of square cross section 0.9525 cm. on a side. This cross

section is chosen because it is a stock size (¾ in.) and is the largest rectangular rod that will fit in the better permeameters already mentioned. It is a very satisfactory size both magnetically and mechanically.

In the present brief paper we have had time for but a glance at some of the problems of magnetic testing, but I trust that I have succeeded in showing that we have done something at the Bureau toward the securing of uniformity in magnetic testing, especially along the lines of:

1. Magnetic treatment during the test.
2. Conditions under which the induction shall be measured.
3. Limits of induction in a standard hysteresis loop.
4. Size of specimen.

Uniformity in the magnetic conditions under which the test is made is absolutely necessary if the data of different experimenters are to be comparable. For purposes of comparison also, prescribed limits for the hysteresis loop are desirable. A standard size and shape for the specimen would add nothing to the accuracy of the test, but would cut the cost of test down to a fraction of what it would be if the testing laboratory has to handle all sizes and shapes.

DISCUSSION.

Mr. C. E. Skinner.—I do not know that I have anything in particular to add except to emphasize the desirability of adopting uniform specifications and uniform methods, and, further, the desirability of having these specifications such that the mimimum amount of testing may be done for any particular result desired. Magnetic material is used under a large number of conditions, and entirely different data may be required in the case of one use from that of another. In one instance we may wish to use the material at very high inductions—say, 17,000 or 18,000 lines in place of 10,000, this induction being entirely beyond the saturation point. In another case we may wish to use the same material at inductions much below 1,000, and any information in regard to the material higher than 1,000 is superfluous in this latter case. The necessity for accurate determination of magnetic properties of materials is increasing, as in design work it is becoming more and more necessary to consider every possible economy in the use of material. I can well remember when it was not necessary to pay any attention to the quality of cast iron used in the frame of the average dynamo, as the amount of material used for mechanical reasons was far in excess of that required for magnetic reasons; and, furthermore, by far the greater part of the total reluctance of the circuit is located in the air gap between the field poles and the armature, so that the quality of the cast iron in the field becomes of importance only when the amount is reduced to the smallest possible proportion. It is particularly desirable that a quick and accurate method be provided for measuring magnetic permeability, as satisfactory results on any given grade of material can only be obtained after the making of many measurements. In establishing specifications, various limits will also have to be provided on account of the wide variation in the conditions under which such material is used. **Mr. Skinner.**

Mr. G. B. Waterhouse.—I should like to inquire as to the size of test rod that will fit the different types of commercial instruments. **Mr. Waterhouse.**

Mr. Burrows. MR. C. W. BURROWS.—A rod $\frac{3}{8}$ in. square will fit in any of these instruments. That is the largest size that will fit in any one of them. From the purely magnetic point of view I prefer to use a rod as small as we can have. From the mechanical standpoint, which we are bound to consider, it must be made larger. It is a pretty difficult matter to get a small rod of uniform cross section and I think there would be no difficulty in preparing rectangular rods $\frac{3}{8}$ in. square and 14 in. long. Mechanically a shorter rod would do, but I believe that the best magnetic work could not be done on a rod much shorter than fourteen inches.

Mr. Capp. MR. J. A. CAPP.—I don't happen to have used any instrument like those described in the paper. We have constructed our own permeameters, and we find that the circular bar, being so much more easily made, is more available for our work. We have a series of three different sizes. We use one $\frac{5}{16}$ in. in diameter and about $6\frac{1}{2}$ ins. long, one $\frac{1}{2}$ in. in diameter and 12 ins. long, and one an inch in diameter and 20 ins. long. For materials which are of our own manufacture, we can, of course, make our test piece of suitable size, but when we get samples from outside we test what is given to us and select that size of test piece which the sample will yield.

As to Mr. Burrows' remarks on the desirability of uniformity, I can heartily second them, because we have brought to us a great many curves which manufacturers of material have developed with the idea of desirable magnetic qualities, and usually their results are utterly incomprehensible. They fail to give us fundamental data. They give us the curve with figures on it, though they themselves don't know what the figures are. The men who present them to us are, as a rule, not electricians, and they cannot help us, yet they have gone to considerable expense in getting data which after they have got it is not worth anything. If some sort of uniformity could be established in the dimensions of the test pieces, but more particularly in the statement of results, it would greatly help us who are trying to use these results.

An important point generally overlooked in reports on magnetic quality materials is a detailed statement of the way in which the sample was obtained, the size of the piece from which taken, as well as its location in that piece. Of even greater importance is the history of the sample with respect to heat treatment or any other manipulation to which it may have been subjected.

MR. J. W. ESTERLINE (by letter).—I have read with much interest the paper by Dr. Burrows on "Uniformity in Magnetic Testing." He has outlined clearly the requisites for the accurate determination of standard test bars, a thing which is very essential so long as we use permeameters which depend upon standard bars for calibration. Mr. Esterline.

My experience in testing straight bars by means of the Ewing double yoke method has led to the conclusion that the reluctance in the yokes is due not so much to the joint between the bar and the yoke, as to that portion of the bar included within the yoke. This reluctance varies with different materials and of course with the density in any material. As the density is increased, the flux is crowded more and more into the bar, as shown in Fig. 1. To my

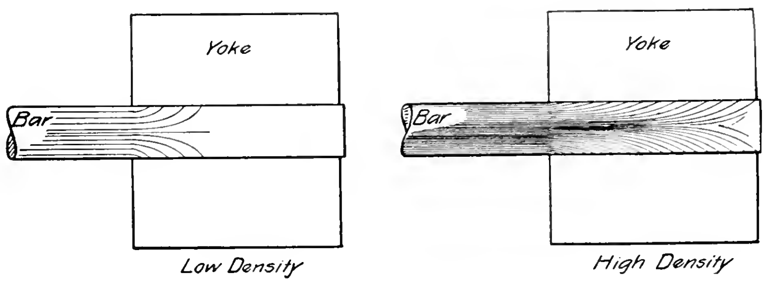


FIG. 1.

mind the greatest objection to the double yoke method is the difficulty of obtaining two samples which are mechanically and magnetically identical.

A year ago we tried the scheme of using a single bar, with its ends joined by means of a heavy soft iron yoke. This apparatus is shown in Fig. 2, and the diagram for it in Fig. 3. The two magnetizing coils *b b* were separated just enough to get the exploring coil *o* on the bar. The induction in this coil is balanced by that in the secondary of an air core solenoid, *i*. The coils *c c* are the compensating coils provided to overcome the reluctance of the joints and the yoke. To indicate when the compensation is properly adjusted, a small magnetic needle *d*, is mounted just opposite the end of the bar. When there is a difference of potential magnetically between the bar and the yoke, the needle moves off zero. The compensating current is then adjusted until the needle returns

Mr. Esterline. to its normal position. This method, we find gives very consistent results, and being a zero method, any sensitive instrument will serve as a galvanometer.

I wish to correct one statement of Dr. Burrows', to the effect

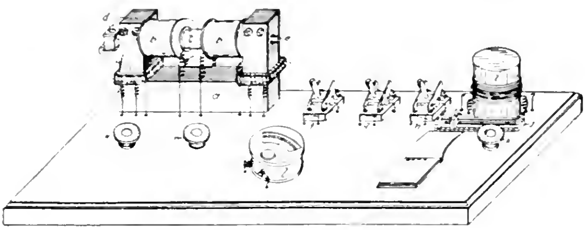


FIG 2.

that the permeameter ascribed to me requires the use of a correction curve, as do the Koepsel and Aryton instruments. The circuits of this instrument have never been published, which will account for this assumption on the part of Dr. Burrows.

In this machine, the E. M. F. developed by the armature is directly proportional to the flux threading the circuit, and in this

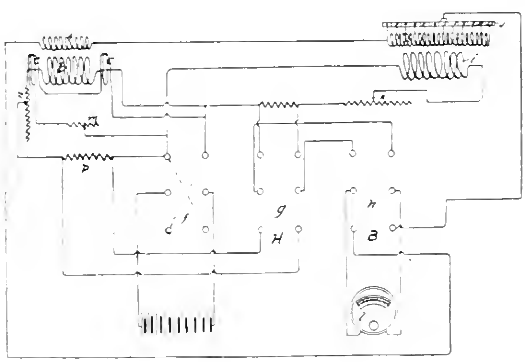
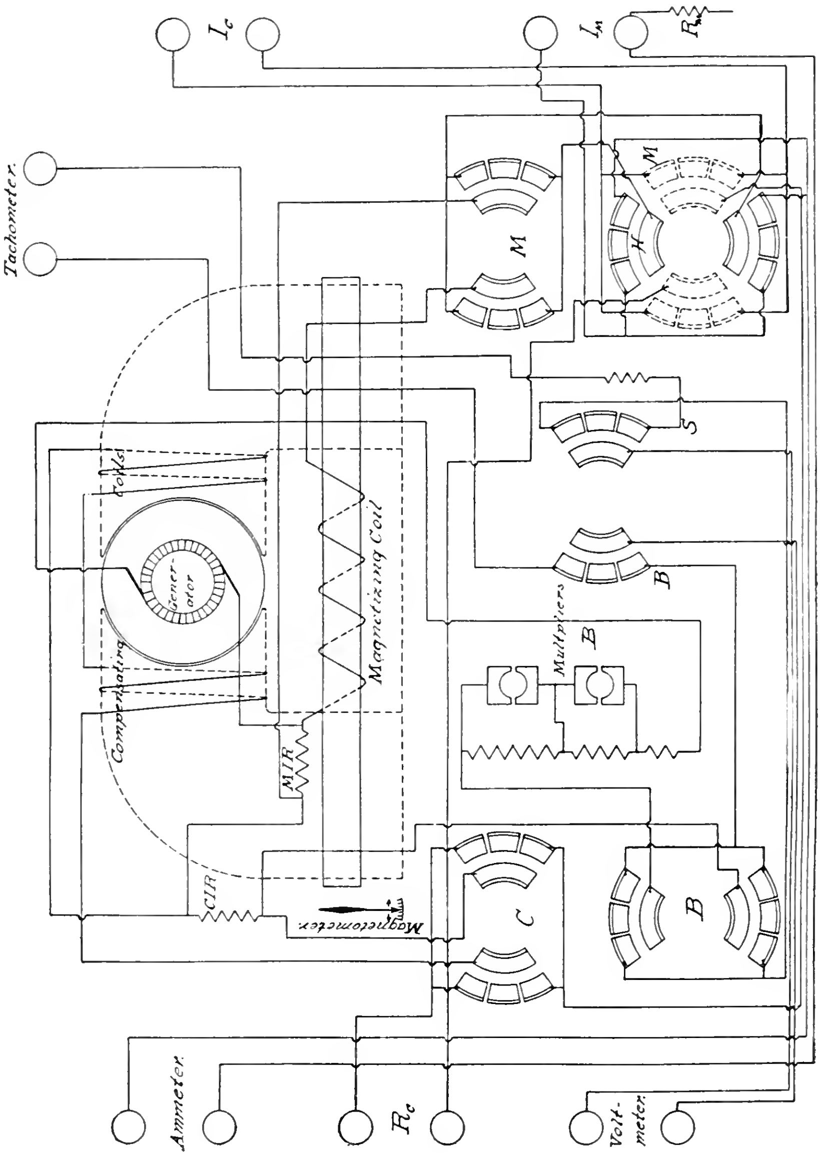


FIG. 3.

respect the operation of the apparatus is similar to that of Koepsel, except for the substitution of the rotating armature for the coil. The use of an armature, the E. M. F. of which indicates the magnetic flux, has several advantages over the use of the coil, viz., the possibility of using multipliers for changing the sensibility of



Tachometer.
I_c
I_m
R_m
M
M
H
S
B
Multipliers
B
B
C
Magnetizing Coil
Gener-
ator
Coils
Compensating
MIR
CIR
Magnetometer.
Ammeter.
R_c
Volt-
meter.

FIG. 4.

Mr. Esterline. the voltmeter, and the automatic correction for leakage, demagnetizing action and stray flux, as will be shown later.

Referring to the diagram, Fig. 4, the poles and armature of the generator are shown in the outline. The magnetizing current enters at the binding posts *Im* through an adjustable resistance *Rm* and flows to the binding posts marked *Ammeter*, to which is connected an ammeter graduated to read directly in values of *H* or ampere turns per inch, for the magnetizing coil.

The current is then led to the switch *A*, which performs the

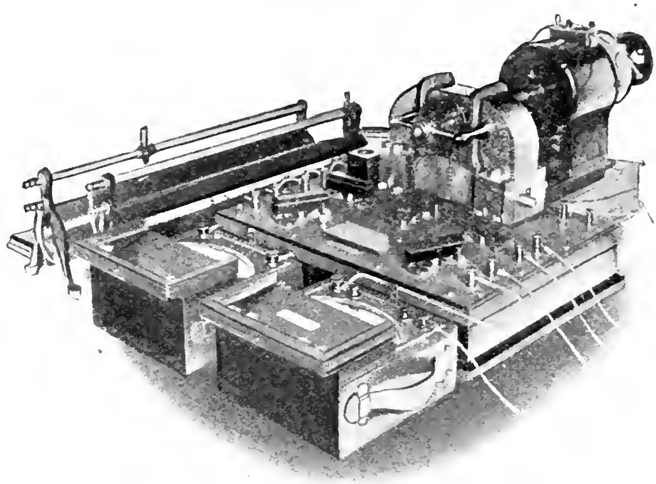


FIG. 5.

double function of reversing both the magnetizing and compensating currents. From this switch it flows to the switch *M*, which is provided to reverse the magnetizing current only, thence through the magnetizing coil, and a fixed resistance *Mir* (the use of which will be explained later), and to the source.

As shown in Fig. 5 the compensating coils are placed about the poles, near the air gap; the current for these enters at the binding posts I_c and flows thence directly to the switch *A*, then to the binding posts R_c, to which is connected an adjustable rheostat, thence to the switch *C* which can be used to reverse the compensating current independently of the magnetizing current. then through

the compensating coils and a fixed resistance *Cir* (the use of which is shown later), and back to the source. Mr. Esterline.

To assist in maintaining a constant speed, an electric tachometer which generates a direct current E. M. F. is coupled to the shaft of the motor. The leads from the brushes of the tachometer are connected to the binding posts marked "tachometer," which are in turn connected to the double throw switch. When the arm of this switch is thrown to the position *S*, the voltmeter connected to the binding posts marked *Voltmeter* indicates the speed of the armature.

One of the leads from the brushes on the commutator of the permeameter armature is connected directly to the switch *B* through a series of multipliers with plugs. The other lead is connected to the same switch, through the two fixed resistances *Mir* and *Cir*. The purpose of the switch *B* is to reverse the potential on the voltmeter, when reading density, so that the reading can be obtained when the magnetizing current is reversed. From the switch *B* one lead goes directly to the "voltmeter" binding post and the other is carried to the double-throw switch, so that throwing the double-throw switch to the position *B* causes the voltmeter to read magnetic density. In this manner a single voltmeter is made to serve the twofold purpose of flux and speed indicator.

Immediately opposite the end of the test bar is placed a small, sensitive magnetometer, adjusted so that when there is no current in the magnetizing and compensating coils, and no bar in the apparatus, the needle stands at zero.

The principle upon which the apparatus operates is as follows: When a magnetizing action is produced by the current in the magnetizing coil, the flux passes through the poles and armature and generates an E. M. F. proportional to the flux through the armature. Operating without compensating action the apparatus would be subject to three errors, viz., magnetic leakage, the reduction in the effective E. M. F. of the coil, due to the reluctance of the joints, poles, air-gap and armature, and the voltage generated by the flux which is set up through the armature, but which does not pass through the test piece.

Since there is some reluctance in the magnetic circuit, other than that of the bar itself, there is a difference of magnetic potential between the ends of the bar, so that the flux set up by this potential

Mr. Esterline. will cause the needle of the magnetometer to deflect. Now by passing a current of the proper strength through the compensating coils, in the right direction, the needle of the magnetometer may be brought to zero, which condition exists when the magnetizing action of the compensating coils is sufficient to overcome the reluctance of the poles and air-gaps for the amount of flux passing through them. The bar is then in a sense short-circuited by a magnetic circuit of zero reluctance, so that the magnetizing force produced by the magnetizing coil may be considered as acting on the free length of the bar only. The effect of the compensating coils in reducing the reluctance to zero also effectually overcomes the leakage.

The flux set up by the magnetizing and compensating coils which does not pass through the bar, but which is cut by the armature, is corrected for in the following manner: Experiment has shown that these fluxes are proportional to the values of the currents in the respective coils and therefore the voltage developed by them will be proportional to the currents. The fixed resistance *Mir* is so proportioned that the drop around it when carrying the magnetizing current is just equal to the voltage developed by the armature when cutting the stray flux set up by the magnetizing coil, when there is no bar in the machine. The magnetizing current is passed through the resistance *Mir* in a direction such that the drop around this resistance opposes the E. M. F. set up by the armature. Since the drop about this resistance and the voltage produced by the stray flux are both proportional to the magnetizing current, equal to each other, and opposite in direction, no reading is produced on the voltmeter by the stray field. The effect of the stray field set up by the compensating coils is overcome in a similar manner by the resistance *Cir* placed in the compensating circuit. In this manner the errors are eliminated and the apparatus indicates directly the flux produced in the bar by the magnetizing coil, while the ammeter reads the corresponding value of magnetizing force.

The apparatus is usually provided with holes for testing round bars and rectangular openings to receive square or rectangular bars. Sheet steel specimens are tested by cutting a number of strips of the same width, these being clamped in the same manner as a rectangular bar.

By increasing the number of multipliers in the voltmeter circuit, a single machine can be made to read directly the magnetic density in a number of different specimens. For steel mills and manufacturers where a large number of different cross sections are to be tested, the apparatus is made to read the total flux in the bar, the density being determined by dividing the total flux by the area of cross-section of the specimen. **Mr. Esterline.**

OFFICERS

OF THE

AMERICAN SOCIETY FOR TESTING MATERIALS.

PRESIDENT,

CHARLES B. DUDLEY.

VICE-PRESIDENT,

R. W. LESLEY.

SECRETARY-TREASURER,

EDGAR MARBURG.

Office: University of Pennsylvania, Philadelphia, Pa.

MEMBERS OF THE EXECUTIVE COMMITTEE:

Term Expiring in 1909.

W. A. Bostwick, Wm. R. Webster.

Term Expiring in 1910.

James Christie, Henry M. Howe.

STANDING COMMITTEES.

COMMITTEE ON FINANCE.

Wm. R. Webster, *Chairman*, W. A. Bostwick.
R. W. Lesley.

COMMITTEE ON MEMBERSHIP.

James Christie, *Chairman*, R. W. Lesley,
Edgar Marburg.

COMMITTEE ON PUBLICATIONS.

Henry M. Howe, *Chairman*, W. A. Bostwick.
Edgar Marburg.

PAST OFFICERS.

NOTE.—The Society, from its organization in 1898 till its incorporation under its present name in 1902, was designated the American Section of the International Association for Testing Materials.

The officers and members of the Executive Committee during this four-year period were as follows:

CHAIRMEN:

MANSFIELD MERRIMAN, 1898–1900.

HENRY M. HOWE, 1900–1902

VICE-CHAIRMEN:

HENRY M. HOWE 1898-1900.

CHARLES B. DUDLEY 1900–1902.

SECRETARIES:

RICHARD L. HUMPHREY, 1898–1900.

J. M. PORTER, 1900–1902

TREASURERS:

PAUL KREUZPOINTNER, 1898–1900.

R. W. LESLEY, 1900–1902

MEMBERS OF EXECUTIVE COMMITTEE:

GUS. C. HENNING. 1898–1900.

ALBERT LADD COLBY, 1900-1902.

STANDING ADVISORY COMMITTEES.

On Iron and Steel.

William R. Webster, *Chairman.*

C. Kirchhoff, H. V. Wille.

On Cast Iron.

Richard Moldenke, *Chairman.*

Henry Souther, Thos. D. West.

On Cement and Concrete.

Clifford Richardson, *Chairman.*

Richard L. Humphrey, Spencer B. Newberry.

On Brick and Terra-cotta Products.

Edward Orton, Jr., *Chairman.*

H. O. Hofman, W. D. Richardson

On Preservative Coatings.

G. W. Thompson, *Chairman.*

Robert Job, S. S. Voorhees.

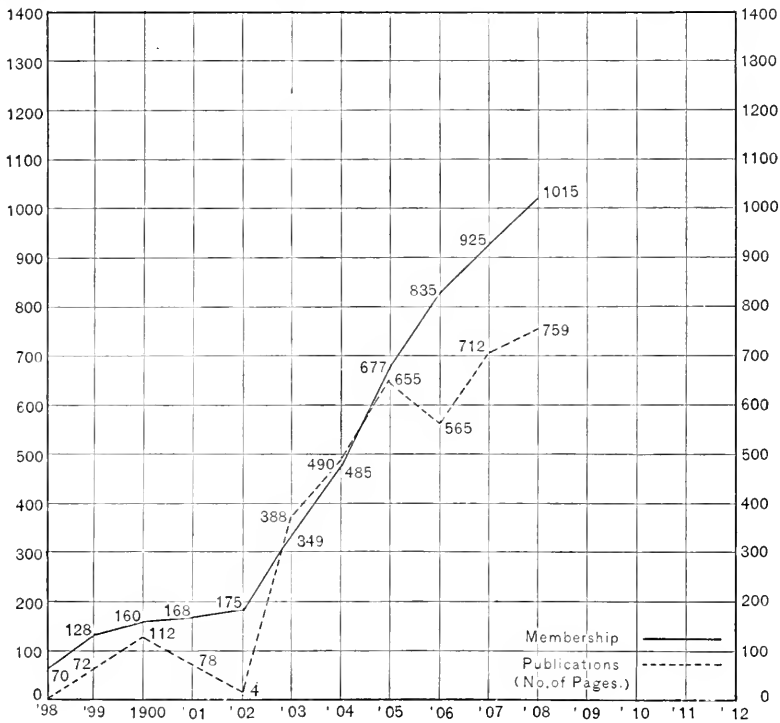
On Tests and Testing Apparatus.

Gaetano Lanza, *Chairman.*

Mansfield Merriman, Tinius Olsen.

ANNUAL REPORT OF THE EXECUTIVE COMMITTEE.

Since the Tenth Annual Meeting of the Society the Executive Committee has held four regular meetings, of which one was informal, owing to the absence of a quorum. An abstract of the Minutes of these meetings is appended to this report.



The record of the Society for the past year is highly encouraging. Volume VII of the Proceedings is the largest volume ever issued by the Society, and its contents are otherwise fully up to the standard of previous publications. To stimulate the activity of the standing technical committees, a general meeting of these committees was held at Columbia University, New York, in January,

with good results. The net growth of membership for the year, after deducting the loss of 64 members, by death, resignation and delinquency, is 90, which is the same increase as for the year preceding. In view of the depressed business conditions during the past year this record is particularly gratifying, and indicative of the firm position to which the Society has attained. There can be no doubt, however, that with the active, individual support of the large membership—now past the thousand mark—the future growth of the Society should progress even more rapidly.

At the invitation of the Executive Committee a number of Government branches, prominently interested in the testing of materials, were represented at a recent meeting of the Committee to consider the desirability of establishing closer relations with the Society, and the means by which this might best be accomplished.

On the part of the Society attention was drawn to the desirability of:

1. The presentation of brief reports at the annual meetings of the Society summarizing the activities during the year of the several Government branches in matters relating to the testing of materials.

2. The presentation at the annual meetings of the Society of papers by representatives of the various Government laboratories relating to the results of investigations of special interest on the properties of materials conducted during the year.

3. Coöperation between the various Government laboratories and the numerous standing technical committees of the Society by means of correspondence, visits, and by making the laboratory facilities available, if feasible, for purposes of investigation in connection with subjects under consideration by these committees.

The Government representatives present expressed themselves as personally in full accord with the spirit of these proposals, and it was the unanimous sense of the meeting that such coöperation would be of mutual advantage to all concerned.

The President and Secretary of the Society were instructed to address communications to the heads of the various Government branches concerned with the testing of materials with a view of acquainting them with the general tenor of the discussion at this conference, and respectfully inviting their coöperation in the directions indicated.

In pursuance of this action, communications were addressed to the heads of the following Government branches:

Bureau of Chemistry,
Bureau of Forestry,
Bureau of Standards,
Bureau of Steam Engineering,
Office of Public Roads,
Office of Supervising Architect,
Panama Canal Commission,
Technologic Branch, United States Geological Survey,
Washington Navy Yard,
Watertown Arsenal.

The replies received evince a general and highly gratifying desire to coöperate with the Society in the directions above indicated so far as circumstances may permit. The immediate effect is seen in the marked enrichment of the program of the present meeting by contributions from representatives of various Government bureaus.

Among the minor betterments for the year may be mentioned the adoption of the modern loose-leaf ledger system of bookkeeping at a substantial outlay for new books and extra clerical services; and the acquirement of an apartment in a fireproof storage building for the safe and orderly deposit of the Society's stock of publications—an asset of constantly growing value. The proceeds from the sale of publications for the past year amount to $1,073.05 which represents an increase of over 50 per cent. above the highest previous record.

Attention is called to the fact that the supply of certain volumes is likely to be exhausted in the measurably near future, so that members desiring to acquire complete files may place their orders promptly. The present inventory, in round numbers, is as follows:

Vol. I, 300 copies; Vol. II, 250 copies; Vol. III, 170 copies; Vol. IV, 210 copies; Vol. V, 350 copies; Vol. VI, 780 copies; Vol. VII, 650 copies. Members connected with institutions of learning, or otherwise interested in public libraries, are especially advised to take early action towards the acquirement of complete library files while these are yet obtainable.

Membership.—The membership at the last annual meeting

was 925. Since then 154 new applications have been received and approved. The Society has suffered a loss of four members through death: D. Woodman died September 4, 1907; P. T. Austen died December 30, 1907; S. H. Ludlow died January 16, 1908, and F. C. Warman died April 27, 1908. The number of resignations for the year is 31, and 29 members have been dropped for arrears in dues. The total loss from all causes is 64, leaving a net gain of 90, and making the total membership at present 1,015.

Publications.—In addition to the annual volume of the Proceedings (Vol. VII, 759 pp.), a pamphlet of 113 pages containing the list of members and other information of a general nature concerning the Society was issued, as well as five official circulars of information.

Technical Committees.—Committee O, on Uniform Speed in Commercial Testing, having completed its investigations and presented its final report at the Tenth Annual Meeting, has been discharged. In pursuance of action at the last annual meeting a committee on Standard Specifications for Coal, to be designated as Committee O, is now in course of organization.

Finances.—The financial condition of the Society, while gradually improving, is not entirely satisfactory. The cost of printing and clerical services for the past year has been considerably larger than for any previous year, due partly to conditions of a temporary nature already alluded to. The cash balance shown in the subjoined report of the Treasurer is $285.23, whereas the outstanding obligations aggregate $493.06, leaving a small deficit for the year ending June 15, 1908. The estimated receipts from all sources for the balance of the current fiscal year ending December 31, 1908, are $2,000, which sum will probably not suffice to meet running expenses. Contrary to the expectations expressed in the last annual report, it will probably be found necessary to call again on the support of the contributing members.

In the judgment of the Executive Committee the financial situation would be greatly improved by changing the beginning of the fiscal year from January 1 to July 1. A proposed amendment of the By-Laws to that end has already been announced. The adoption of this amendment will render members liable for six months' dues on January 1, 1909, and for one year's dues on July 1, 1909, which will serve to relieve the treasury during the second

half of the calendar year when the expenditures for printing are heaviest, and enable the Society to meet its financial obligations during that period promptly, which has been found impossible in the past. Incidentally the close of the fiscal year will then be practically coincident with the dates of the Annual Meetings, which may be expected to have a good effect on resignations and delinquencies, which amounted to a total of 60 last year, and 66 the year before.

ANNUAL REPORT OF THE TREASURER.

From June 15, 1907, to June 15, 1908.

RECEIPTS.

Membership dues	$5,456 51	
Contributing membership dues	50 00	
Collections for account International Association	422 25	
Sale of publications	1,073 05	
Orders for binding	439 38	
Reprints	150 12	
Interest on deposits	13 74	
Miscellaneous receipts	20 53	
Total receipts	$7,625 58	
Cash balance June 15, 1907	268 80	
		$7,894 38

DISBURSEMENTS.

Remitted to International Association, dues, etc	$76 90	
Expenses, International Association	1 50	
Printing, engraving, binding, etc	4,145 89	
Secretary's salary, July 1, 1907, to May 31, 1908	1,375 00	
Clerical services	1,355 88	
Expenses, Secretary's office	396 08	
Stenographer, Tenth Annual Meeting	175 00	
Expenses, Tenth Annual Meeting	52 52	
Committee expenses	7 88	
Excess remittances refunded	22 50	
Total disbursements	$7,609 15	
Cash balance, June 15, 1908	285 23	
		$7,894 38

Relations with the International Association for Testing Materials.—Of the 1,015 members of the American Society, 267

hold membership in the International Association, representing an increase of 90 in the former and a loss of 2 in the latter for the year. The Fifth Congress of the International Association will be held in Copenhagen in 1909. In view of the fact that the Association, in connection with its last congress, inaugurated the policy of publishing its official reports in three languages and twenty-seven such reports were issued, besides a large number of non-official papers, it is believed that the members of the American Society will find membership in the International Association highly advantageous. The annual dues are $1.50 and application blanks may be obtained from the Secretary of the American Society.

It is to be hoped that the American participation in the next Congress, in the way both of papers and attendance, will show a marked improvement over the unsatisfactory record at the Fourth Congress held at Brussels in 1906.

The Executive Committee has authorized a subscription of 100 francs for the current year towards the treasury of the International Association.

The Council announces that, owing to the death of Professor Giessler, Editor of *Baumaterialenkunde*, the publication of that journal, which has served as the official organ of the International Association since 1896, was discontinued at the end of the year 1907.

Respectfully submitted on behalf of the Executive Committee,

CHARLES B. DUDLEY,
President.

EDGAR MARBURG,
Secretary-Treasurer.

APPENDIX.

ABSTRACT OF MINUTES OF THE EXECUTIVE COMMITTEE.

Regular Meeting, July 9, 1907.—Pennsylvania Building, Philadelphia, Pa. Present: Messrs. Dudley, Lesley, Webster and Marburg.

The Secretary reported the receipt of 48 new applications for membership, duly approved, and 3 resignations, making the total membership on June 15, 1907, 925.

The Secretary presented a report from the Auditing Committee, consisting of Mr. R. W. Lesley and Mr. J. A. Colby, certifying to the correctness of the books and accounts from January 1, 1907, to June 15, 1907, the date of the annual report of the Treasurer, and of the cash balance of $268.80.

The question of appointing a committee on Standard Specifications for Coal, which was referred to the Executive Committee at the last Annual Meeting, was discussed, and it was decided to authorize the President and Secretary to proceed with the organization of such a committee, with the understanding that Dr. J. A. Holmes be invited to accept the temporary chairmanship, and that every effort be made to appoint a large and thoroughly representative committee.

The Secretary was authorized to engage a suitable room in a fireproof storage building for the permanent storage of back publications.

It was decided to nominate Mr. William R. Webster and Mr. Walter Wood for appointment by Mr. A. Rieppel of the International Association as the American representatives on the proposed International Committee on Specifications for Iron and Steel, with the understanding that the action of these representatives be subject to the approval of the Executive Committee, and that the appointments be made subject to renewal on January 1, 1908.

Regular Meeting, October 4, 1907.—Pennsylvania Building, Philadelphia, Pa. Present: Messrs. Lesley, Webster and Marburg.

In the absence of a quorum the meeting was declared informal, with the understanding that any action taken would be subject to the approval of the Executive Committee at its next meeting.

The Secretary reported the receipt of 48 new applications for membership, duly approved, and 2 resignations, making the total membership on October 3, 1907, 968.

The Secretary submitted the report of the Tellers, Mr. C. M. Mills and Mr. J. A. Colby, on the letter ballot of the Society covering the adoption of Standard Specifications as follows:

Number of ballots cast: 164, of which three ballots were unsigned and therefore not counted, making the total vote 161, distributed as follows:

Standard Specifications for Steel Rails,	not voting	18	
	for	125	
	against	18	
			161
Definitions of Standard Defects,	not voting	17	
	for	144	
	against	0	
			161
Standard Names for Structural Timbers,	not voting	16	
	for	145	
	against	0	
			161
Standard Specifications for Bridge and Trestle Timbers,	not voting	18	
	for	143	
	against	0	
			161
Standard Test for Fireproof Construction	not voting	17	
	for	139	
	against	5	
			161

The Secretary reported the purchase of a complete set of account books with special ruled pages, including a loose-leaf ledger, with a view of placing the bookkeeping on a more satisfactory basis; also the renting of a room in a fireproof storage building for the safer storage of back publications, at a cost of $10.00 a month.

The Secretary was instructed to insert the following notice in connection with the report of Committee A, on Standard Specifications of Steel Rails, in Volume VII of the Proceedings:

"The Specifications embodied in the above report were referred to letter ballot of the Society by two-thirds vote at the Tenth Annual Meeting, and adopted by a vote of 125 affirmative and 18 negative, canvassed on August 26.

"The Standard length of finished rails was originally fixed at 30 feet instead of 33 feet by the Committee, on account of the then existing scarcity of cars, especially in the East, of suitable length for the shipment of longer rails. This condition having disappeared, the Executive Committee, acting on the recommendation to that effect from Committee A, confirmed by the members of that Committee by letter ballot without a dissenting vote, has authorized the adoption of the 33-foot length in the Standard Specifications for Steel Rails, since the change is one not affecting the quality of the product but dictated by practical considerations only;

this action to be subject to the approval of the Society at the next Annual Meeting.

"The Standard Specifications embodying this change and the corresponding modifications in paragraph 4, on Finishing Temperature, and in paragraph 6, on Shorter Acceptable Lengths, follow in succeeding pages."

The question of coöperation of the Society with certain Government Bureaus interested in the testing of materials was discussed, and it was decided to invite those bureaus to delegate representatives to the next meeting of the Executive Committee for the discussion of this subject.

The desirability of arranging for a general session of the standing technical committees of the Society in New York in January was discussed, and the Secretary was instructed to communicate with the chairmen of the various committees with a view of ascertaining their opinion as to the desirability of holding such a meeting, and to make arrangements accordingly.

REGULAR MEETING, January 6, 1908.—Engineers' Club of Philadelphia, Philadelphia, Pa. Present: Messrs. Dudley, Lesley, Christie, Webster and Marburg.

The action taken at the preceding meeting in the absence of a quorum was approved.

The Secretary reported the receipt of 15 new applications for membership, duly approved, the resignation of 5 members and the dropping of 1 member, making the total membership on January 2, 1907, 977.

The Secretary submitted correspondence with Mr. Paul Kreuzpointner, Chairman of Committee O, on Uniform Speed in Commercial Testing, from which it appeared that the Committee had completed the work for which it was originally appointed. It was accordingly decided that this committee be discharged.

The Secretary submitted communications dated December 16 and 26 from the International Association, and he was instructed to cover the following points in replying to these communications:

1. To state that the Executive Committee has no knowledge of any promise on its part on behalf of the American Society of a subscription of 100 francs for 1907, but to forward such a subscription.

2. To call attention to the efforts made on the part of the Executive Committee towards stimulating interest in the affairs of the International Association.

3. To explain why, in the opinion of the Executive Committee, the American membership in the International Association is not larger.

4. To express the satisfaction of the Executive Committee with the management of the last Congress of the International Association, especially in the matter of publications.

5. To express regret at the death of Professor Giessler, and the consequent discontinuance of the official organ of the Association.

The Secretary was authorized to place an insurance policy of $3,000.00 on the stored publications, for one year, at a premium of $21.60.

The Secretary was authorized to purchase the electrotype plates for Volume VI of the Proceedings at the price of 10 cents per plate, with an additional charge covering cost of boxing, with the understanding that the total charge would be about $70.00.

The Secretary was instructed to arrange for a general meeting of the technical committees of the Society at Columbia University, January 28 and 29.

Regular Meeting, April 6, 1908.—Engineers' Club of Philadelphia, Philadelphia, Pa. Present: Messrs. Dudley, Lesley, Christie, Webster and Marburg.

Before considering the regular business of the meeting a conference was held with the representatives of certain Government Bureaus interested in testing, who had been invited to attend the meeting with a view of considering means of coöperation to mutual advantage. Invitations had been extended to representatives of the following Government Branches:

Bureau of Forestry,
Bureau of Standards,
Office of Supervising Architect,
Road Material Laboratory,
Technologic Branch, United States Geological Survey,
Watertown Arsenal.

There were present at the meeting:

Mr. McGarvey Cline, of the Bureau of Forestry,
Dr. W. K. Hatt, of the Bureau of Forestry,
Mr. D. T. Randall, of the Technologic Branch, United States Geological Survey.
Dr. S. W. Stratton, of the Bureau of Standards,
Major C. B. Wheeler, of the Watertown Arsenal,
Mr. S. S. Voorhees, of the Office of the Supervising Architect.

The Secretary read a telegram from Dr. A. S. Cushman, who was expected to represent the Road Material Laboratory, to the effect that he found himself unable to attend the meeting. The ensuing discussion was entirely favorable to the general proposition, and, on motion, the President and Secretary were instructed to address a communication embodying the sense of the meeting, to the heads of the Government Bureaus interested in testing.

The Secretary reported the receipt of 53 new applications for membership, duly approved, 19 resignations, and the deaths of P. T. Austen on December 30, 1907, and S. H. Ludlow on January 16, 1908, making the total membership on April 6, 1908, 1009.

The Secretary was instructed to advise delinquent members that they would be granted an extension of time till May 1, 1908.

The Secretary was authorized to fix the prices of Standard Specifications as follows: To non-members, 25 cents for a single copy, $3.00 for a complete set of 20; to members, 15 cents for a single copy, $2.00 for a complete set of 20.

The following proposed amendments to the By-Laws were, on motion, approved, subject to the provisions in the By-Laws covering amendments:

Procedure Governing the Adoption of Standard Specifications.

Article IV.

Section 1. A proposed standard specification must be presented at the Annual Meeting, at which it may be amended by majority vote of those voting. A two-thirds affirmative vote of those voting shall be required to refer the specification to letter ballot of the Society. A two-thirds affirmative vote of those voting on letter ballot shall be required for the adoption of the specification.

Change the enumeration of Articles IV and V to Articles V and VI, respectively.

Amend Article V (old Article IV), Section 1, by striking out "January" and substituting "July."

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